THE CENTURY SCIENCE SERIES

JUSTUS VON LIEBIG

HIS LIFE AND WORK

SHENSTONE

Edited by
Sir Henry E. Roscoe
D.C.L., LL.D., F.R.S.

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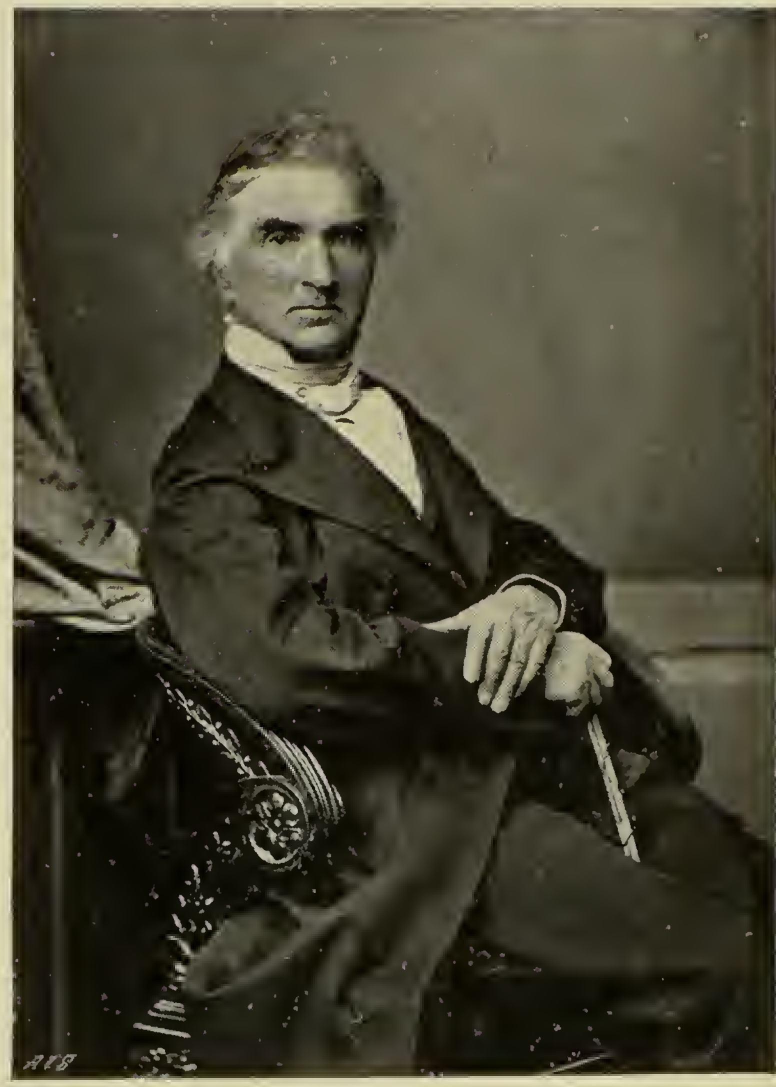

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JUSTUS VON LIEBIG.

Born May 12, 1803. Died April 18, 1873.

THE CENTURY SCIENCE SERIES

JUSTUS VON LIEBIG

HIS LIFE AND WORK

(1803—1873)

BY

W. A. SHENSTONE, F.I.C.

Lecturer on Chemistry in Clifton College

CASSELL AND COMPANY, LIMITED

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LA BELLE SAUVAGE

PREFACE.

The name of Liebig is doubtless familiar to most of us, but I fear that very few have any clear idea what he did, why chemists admire and esteem him, or, indeed, are aware that they do admire and esteem him. As the result of many inquiries, made among cultivated people, I have found the prevailing impression concerning Liebig to be that he was a man who gained a large fortune by making "extract of meat." Now and then one meets someone who "seems to have heard" of his name in connection with agriculture. Scarcely anyone, now, seems to know that he was one of the greatest of that class in whose work Mr. Balfour finds "the causes which, more than any others, conduce to the movements of great civilised societies." I have therefore made it my object, in writing this little book, not so much to dwell upon Liebig's private life as to tell what he was, what he did, and why all chemists and all those who are versed in the history of science admire and esteem him so greatly.

Fortunately for my purpose, most of Liebig's work is not only of great general interest, but it lends itself admirably to a non-technical method of treatment. Consequently, I have only found it necessary to employ the language of chemistry in parts of two chapters. As I have been careful to explain technical terms when I have used them, and as I have not very

often employed them, I do not think they will be a real source of difficulty or repel anyone.

If any chemist should read this life of Liebig, he may not improbably feel disposed to complain that it does something less than justice to Liebig's labours in pure chemistry. I admit that this is very true. But it is right that it should be so, for, vast as were Liebig's services to pure chemistry, they lack in some degree the splendour of his contributions to some other departments of equal intrinsic importance and of far wider general interest.

In concluding these few introductory words, I desire to express my thanks to several very kind helpers: to Liebig's son, Dr. Georg Baron Liebig, who has assisted me most graciously in several ways: to my friend and colleague, H. Clissold, who has most carefully read the proofs for me; and to my wife, who has very materially lightened my task by helping me to go through the greater part of the numerous bulky volumes which contain Liebig's published correspondence.

W. A. S.

Clifton, May, 1895.

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JUSTUS VON LIEBIG: HIS LIFE AND WORK.

CHAPTER I.

Introduction—Early Life and Tastes—His Wander-Year—Appointment at Giessen—Method of Organic Analysis—Some other Contributions to Chemical Method.

It is remarkable that in spite of the epoch-making character of Liebig's contributions to chemistry, to agriculture, to physiology, and to the advancement of education, and in spite of the fact that his name is still a household word over a large part of two continents, no comprehensive or popular account of his life and work has yet been written, though it is more than twenty years since death robbed us of one of the greatest men of this or perhaps of any other century.

Of Faraday—who lived and worked like Liebig, one might almost say with Liebig, when, to men of science, the times were young—we have already two lives, those of Dr. Bence Jones and of Dr. Gladstone. Of Pasteur, Liebig's great opponent on the question of the cause of fermentation, whose personality stands out to-day only less distinctly than did that of Liebig fifty years ago, we have a delightful, if rather one-sided, account written by his son-in-law, M. Valery Radot. But of Liebig, perhaps the greatest and the most many-sided of all, we have as yet

only the memorial addresses of August Vogel on his work in agricultural chemistry, of Emil Erlenmeyer on his contributions to pure chemistry, and of Theodor L. W. von Bischoff on his work and influence on physiology, together with the celebrated Faraday lecture of his brilliant and distinguished pupil A. W. Hofmann, some too brief fragments of autobiography, Moriz Carriere's account of his friendship with Platen the poet, and some more fugitive but still interesting contributions, such as Sir Henry Roscoe's obituary notice in *Nature* of May 8th, 1873.

But this apparent neglect is only apparent, and is easy to understand. Liebig, owing to his unrivalled gift of popular exposition, was his own prophet. As he had no need of an interpreter while he lived, so there was no immediate need of a monument after his death. His memorable "Familiar Letters on Chemistry," by means of which he conveyed his teachings to the people, informed those of his own and the succeeding generation what his life-work had been; and they remained as a sufficient memorial of him for many years after he was gone.

But with the progress of science the time has now come, as it was sure to come, when the majority of readers can no longer safely betake themselves to Liebig's own writings, in order to learn the part he played in the development of human knowledge. And it is a question whether the immense value of his services is not already more than half forgotten owing to the absence of any suitable review of his work. This is especially true of his educational work. How many educated men of to-day—nay, how many of the younger chemists—are aware that it may fairly be

said that it was he who paved the way for the educational revolution which will be for long associated with the second half of the nineteenth century by establishing in 1825 at Giessen his famous laboratory for giving instruction to all comers in practical chemistry ?

Justus von Liebig was born on May 12th, 1803, at Darmstadt, where his father dealt in colours, which he also frequently manufactured according to the processes then prescribed in works on chemistry, sometimes with the aid of his small son. As a schoolboy, Liebig was not a success from the pedagogic point of view; his bent of mind was so distinctly that of the experimenter that, as he tells us, his position at school was very deplorable. Like many other lads of this type, he had no ear memory, and could retain little or nothing of what he learned through the sense of hearing, with the result that he found himself in as uncomfortable a position as a boy could possibly occupy. Not only were all the acquirements that led to praise and honour in the school utterly out of his reach, but once the good Rector of the gymnasium, on the occasion of examining Liebig's class, made a most cutting and public remonstrance with him, reproached him for want of diligence, told him he was the plague of his teachers and the sorrow of his parents, and ended by asking him what did he think was to become of him. Liebig, who, though so ignorant in the linguistic studies of the place, was already pretty widely read in science and versed in the operations of chemistry, replied, amid the uncontrollable laughter of the good rector and of the whole school, "That he would be a chemist." No one at that time had any

idea that chemistry was a subject that could be studied for itself. To most it was a mere accessory subject, at best a handmaid to medicine and pharmacy; the idea of the study of chemistry being adopted as a career seemed preposterous.*

It was plain, however, that none of the ordinary careers open to a gymnasium student were possible for Liebig, and owing, doubtless, to his inclination for chemistry, he was taken by his father to an apothecary at Heppenheim. Here he soon became acquainted with the various applications of the multifarious contents of a druggist's shop, but pill-making did not please him; he wished to be a chemist, not a druggist. Soon, therefore, he began to make experiments of a non-pharmaceutical character privately in his attic; before long, accidents occurred, and at last one day the attic window-sash was blown out. At the end of ten months, the alarmed apothecary of Heppenheim was so sorry with his bargain that he sent the lad home again to his father.

Liebig was now about sixteen, at which age in those days most lads were beginning to take life seriously, and it was plainly difficult to know what to do with him, or to foresee what he would do for himself. His time had not, however, really been wasted. The interest he had taken in his father's work had led him long before he left school to read with passionate interest the books used for guidance in the manu-

* The above and many other personal details concerning Liebig's early life are taken, frequently in his own words, from an autobiographical fragment, which was discovered by his son, Dr. Georg Baron von Liebig, a few years ago, and published in the *Deutsche Rundschau* for January, 1891. A translation of this sketch, by Prof. J. Campbell Brown, was read at Liverpool on March 19th in the same year, and published in the *Chemical News* on June 5th and 12th.

facture of colours. In fetching these books from the Court Library he had become acquainted with the Librarian, Hess, through whose kindness he soon had the run of the library, where he read in anyhow fashion such works as Macquer on Chemistry: Basil Valentine's Triumphal Car of Antimony; Stahl's Phlogistic Theory; together with numerous essays and treatises, including the writings of Kirwan and Cavendish. Of course, as he tells us, he did not in this way gain much exact knowledge from his reading, but it led him to attempt to carry out the experiments which he read about as far as his means would allow, and, these being very limited, to make countless repetitions of such of them as he was able to perform, with the result that, boy as he was, he had already acquired, in a considerable degree, that power of perceiving the resemblances and differences between things and between phenomena which he has called sight- or eye-memory.

Every one of the numerous white precipitates known to the chemist has some quality or qualities peculiar to itself which should be recognised by those who have once fully studied it. Liebig's early habit of experimenting helped to develop in him the eye-memory which makes this possible. This power was afterwards possessed by him to such an extent that in later years he was sometimes able to recognise, by their appearances alone, eleven rare chemicals many years after having once worked with them, with such certainty that he was not even misled by the results of the analysis of impure specimens.

Though Liebig had not mastered the lessons of the gymnasium, he had gained a knowledge of most of the processes carried on in his neighbourhood. He had

watched the soap-boiler, and had made soap on his own account. "In the workshop of the tanner and dyer, the smith and brass founder, he was at home and ready to do any hand's turn." Nay, as we shall see, he had even drawn inspiration from a peripatetic cheapjack who visited Darmstadt, and plied his trade in the market place, as he watched him prepare fulminating silver for his pea-crackers, and clean coat collars for the country folk. Left to himself, he had gained extensive stores of information, and a deep-seated desire for more, and at the age of sixteen, by persistent importunity, he induced his father to permit him to go to the University of Bonn, whence he afterwards followed his professor, Kastner, to Erlangen on the removal of the latter to that university.

There had arisen, Liebig tells us, about that time at the then newly-established University of Bonn, an extraordinary quickening of scientific life, which, however, was unfortunately most perniciously affected by the philosophical methods of investigation as they had been embodied by Oken and by Wilbrand, which had led alike in lecture and in study to a want of appreciation of experiment and of an unprejudiced observation of nature that was ruinous to many talented young men—"From the professorial chair the pupils received an abundance of ingenious contemplations; but, bodiless as they were, nothing could be made of them."

At this time Sir Humphry Davy and others in England; Berzelius, the great Swede; and in France a whole galaxy of brilliant experimenters, including Gay-Lussac, Dulong, Arago, and Chevreul, were rapidly opening out new spheres of investigation of almost boundless importance, but their inestimable

acquisitions found no soil, and could bear no fruit in Germany, where, as Liebig says, "It was then a wretched time for chemistry."

At Bonn and Erlangen, therefore, Liebig soon learnt that he was not in the way to become a chemist. But here he also discovered, from his intercourse with other students, his own ignorance of many subjects with which they had gained an acquaintance at school. This was something, and since he could learn no chemistry, he exerted all his energies to make up for his previously neglected school studies; whilst by organising and working with a small band of students, who formed themselves into a chemico-physical union, he gained some practice in composition and in the art of speaking. But this was all, and before long he returned to Darmstadt, persuaded that he could not become a chemist in Germany.

Up to this time Liebig's career had certainly been calculated to give a good deal of anxiety to his relations, especially as latterly he had come into conflict with the authorities, and was even at one time under arrest for supposed political offences, though he was not, as we are told by Platen, conscious of any real fault of his own. But there is no doubt that even at this early stage he showed, to those who were capable of judging him, an extraordinary degree of promise. He had not only attracted the interest of Hess, but he was the favourite pupil of Kastner the chemist, whilst the poet Platen may almost be said to have fallen in love with him at sight. The latter wrote of him in his diary on March 13th, 1822:—"The day before yesterday I made an interesting acquaintance. This is a young chemist from Darmstadt, who is named Justus Liebig, the same student

whom I met some time since at Kastner's. Bülow had already described him to me as Kastner's favourite, as he has, particularly in chemistry, very sound knowledge." Before long Platen and Liebig met again, walked in the country, and afterwards adjourned to Liebig's dwelling-place. Platen afterwards declared his new friend to be clear, definite, and solid in everything, and, above all, on the side of the affections, open and confiding. "Never before," said he, "have I been treated with such affection upon so brief an acquaintance." Owing to various accidents, the meetings of these two were very few, but Platen has left us a delightful description of Liebig's personality at this period. "Liebig," he said, writing after a walk with him, "was never more beautiful. Of slender form, a friendly earnestness in his regular features, great brown eyes with dark shady eyebrows, which attracted one instantly. . . . . Oh that I might, after so many deceptions, find happiness and peace in this friendship, which seems to open up new future possibilities!"

In 1822, at the age of nineteen, Liebig took the degree of Doctor of Philosophy at Erlangen, and at about the same time he published the result of his first attempt at an investigation in a paper on the composition of fulminating mercury, which was remarkable for the clearness and precision of its language. This paper and some analyses of certain colouring matters which he had also already performed fortunately attracted the attention of people who possessed influence with Louis the First, the then reigning Grand Duke of Hesse-Darmstadt. Before long Liebig's chance came. He had the good fortune to be provided by Duke Louis with the necessary means for prosecuting his studies abroad.

But whither should he go?

In those early years of the nineteenth century the younger men among the German chemists had already many of them repaired to Stockholm in search of inspiration and instruction in the modest laboratory of Berzelius. Mitscherlich, the discoverer of isomorphism; H. Rose, the analyst; and, later on, Wöhler, whose production of urea from materials of purely inorganic origin afterwards revolutionised the views of chemists on organic chemistry, and finally established the idea of *isomerism* in the science—all visited Sweden to become the pupils and friends of the great Northern chemist, and it is probable that Liebig, to whom the writings of Berzelius had already been "as springs in the desert," would have followed their footsteps had not Paris offered him opportunities of wider study that were an irresistible attraction. Therefore, to Paris he went, to sit at the feet of the great masters who adorned the French capital at that time. There he found the opportunities he desired. The lectures consisted of a wisely-arranged succession of experiments, whose connection was completed by oral explanations. "The experiments," says Liebig, in his autobiographic fragment, "were a real delight to me, for they spoke to me in a language I understood, and they united with the lecture in giving a definite connection to the mass of shapeless facts which lay mixed up in my head without order and without arrangement," whilst the lectures, as a whole, made a most marked impression on his mind by their intrinsic truth, by the absence of pretence.

At the time of Liebig's arrival in France—that is to say, in 1822—there did not exist in all Paris nor in all the world one such public laboratory for workers

in chemistry or physics as may now be found in every provincial town of the first class. Though the lectures were so excellent, public places of instruction in analysis and experimenting generally were still as completely non-existent in France as in Germany, and admittance into a chemical laboratory was then a difficult thing indeed for a stranger to attain. By the kind assistance of Thénard, however, Liebig was permitted to continue his researches on explosives in the private laboratory of Gaultier de Claubry, and he soon published another paper on this subject. But he was not even then able to feel complete confidence in his results, and he was meditating yet further experiments, when by a happy chance, in the summer of 1823, he made the acquaintance of Humboldt the traveller. From that day Liebig found all doors and all laboratories open to him as by magic, and he was soon at work in the laboratory of Gay-Lussac, revising his analyses of the fulminates, with the result that early in the next year he brought them to a successful conclusion, and was rewarded by the discovery of isomerism and by the honour of a waltz round the laboratory with his distinguished teacher, who was in the habit of relieving his feelings, as he explained to his young friend, when discoveries were made, by such-like terpsichorean exercises.

The account of Liebig's meeting with Humboldt, and of his introduction to Gay-Lussac, was told by him years afterwards to Mr. E. K. Muspratt in the Munich Laboratory, and is so interesting that it deserves to be repeated.

One day in the summer of 1823 he gave an account of his earlier analyses of fulminating silver before the Academy of Sciences. Having

finished his paper, as he was packing up his preparations a gentleman came up to him and questioned him as to his studies and future plans, and, after an exacting examination, ended by asking him to dinner on the following Sunday. Liebig accepted the invitation, but, through nervousness and confusion, forgot to ask the name and address of his interviewer. Sunday came, and poor Liebig was in despair at not being able to keep his engagement.

The next day a friend came to him and said, "What on earth did you mean by not coming to dine with von Humboldt yesterday, who had invited Gay-Lussac and other chemists to meet you?" "I was thunderstruck," said Liebig. "I rushed off as fast as I could run to von Humboldt's lodgings, and made the best excuses I could." The great traveller, satisfied with the explanation, told him it was unfortunate, as he had several members of the Academy at his house to meet him, but thought he could make it all right if he would come to dinner next Sunday. He went, and then made the acquaintance of Gay-Lussac, who was so struck with the genius and enthusiasm of the youth that he took him into his private laboratory, and continued in conjunction with him the investigation of the fulminating compounds.

Nor did Humboldt's assistance stop here, for it was on his recommendation that Liebig was afterwards appointed Professor in the little University of Giessen, where he established a school, whose achievements in pure and applied chemistry must have far transcended the most sanguine of his youthful aspirations. From the most modest beginning and the scantiest means came results which fill one of the most splendid pages in the history of chemistry. It was

in Gay-Lussac's laboratory that Liebig, conscious of what he owed to the guidance and friendship of his master, and conscious too of his own growing insight and power, conceived the idea of founding in Germany a school where he should be to his younger fellow workers that which Gay-Lussac had been to himself. A glorious dream gloriously fulfilled, as we shall presently see.

Liebig was appointed Extraordinary Professor of Chemistry at Giessen in 1824, and Ordinary Professor two years later. He was called to Munich in 1852, and died there on April 18th, 1873.

Liebig was essentially a pioneer in science. In the course of his life he took the lead in no less than four great departures. The first was in organic chemistry, the second and third in the applications of chemistry to agriculture and to physiology, the fourth, as will presently appear, was the outcome of his labours as a teacher. His work, like that of other pioneers, was, of course, not always correct in all points of detail. But it had all the greater merits of good pioneering work in a most marked degree. It almost always pointed the right way, and its remarkable influence in determining the direction of subsequent research has been singularly permanent.

But a pioneer in science, like a traveller, must not set out till he is fully equipped. It was useless for Liebig or anyone else to attempt to explore the depths of organic chemistry before a method for the analysis of organic substances was at his command. This Liebig knew full well, and accordingly we find that his leisure during his first years at Giessen was devoted to contriving such a method.

When the chemist discovers a new substance,

whatever its nature may be, whether it is composed of the so-called mineral elements, such as the metals, sulphur, phosphorus, chlorine, etc., or whether it belongs to the so-called organic group of compounds in which the presence of carbon, oxygen, hydrogen, nitrogen, are especially characteristic, it is necessary in the first place to learn its ultimate composition—that is to say, to find out by analysis what elements it contains, and in what exact proportions they are present.

In organic analysis the chemist has usually only to deal with some eight or ten elements, and often with only three or four—viz. with compounds of carbon, with hydrogen, oxygen, or nitrogen; hence the operations of ultimate organic analysis are characterised by their comparative simplicity; but success in this branch of work has only been attained as the result of many failures, owing to the great initial difficulties which for half a century prevented chemists from making much progress.

Lavoisier was one of the first to attempt to analyse organic compounds. He knew that carbon, when burnt, yields the well-known heavy, suffocating carbonic acid gas, and he had established, with some approach to precision, the relative proportions in which carbon and oxygen enter into its composition.

Similarly from the work of Cavendish, and from his own experiments, Lavoisier knew approximately the relative proportions in which hydrogen and oxygen unite to form water. Armed with these three facts, he endeavoured to ascertain the proportions in which carbon, hydrogen, and oxygen are present in alcohol and oil, by burning weighed quantities of them in small lamps placed under receivers, standing over mercury, to

which such additional measured volumes of oxygen as might be necessary could be added during the process. At the end of the process he measured the carbonic acid gas formed, and the volume of the air which remained. From the data so obtained he endeavoured to calculate the composition of the substance burnt in his lamp; but, unfortunately, neither his knowledge of the composition of the compounds produced, nor of the relative densities of the gases concerned, was sufficiently accurate for his purpose, and hence the results of his experiments had no permanent value. Later he made other experiments, in which he collected and weighed the products of burning oil, but these were cut short by his pseudo-judicial murder in 1794. It was not until Liebig attempted to solve the problem that success was really attained; and Liebig only achieved complete success after devoting many years to thinking and experimenting.

The method of organic analysis given to chemistry by Liebig, like that of Lavoisier, consists in completely burning, or oxidising, the substance which is to be analysed, and then collecting and measuring the carbonic acid gas and water formed. This process is at once admirable in its conception and in its execution. It is a model of simplicity and accuracy. It remains in use, almost unchanged, to this day; it has served for the analysis of countless numbers of organic substances, and has thus formed the very foundation on which the whole vast structure of modern organic chemistry is based. It may fairly be said to be one of Liebig's greatest gifts to science. The details of this beautiful process cannot be here described, but may be found in every treatise on organic chemistry. In contriving it, Liebig selected with unerring judgment

all that was best in the ideas of his predecessors; added his famous absorption apparatus, "Liebig's potash bulbs;" and embodied the whole in a form which remains nearly intact after half a century.

Modifications of Liebig's process that were suitable for bodies containing other elements, such as nitrogen, chlorine, and sulphur were soon introduced, some of them by Liebig or his pupils, and thus the fundamental difficulty which had retarded the progress of organic chemistry to an almost inconceivable extent for nearly fifty years was overcome, and Liebig and his pupils were able to carry out the numerous investigations which have given the little Hessian University a world-wide reputation.

By the analysis of a compound, however, the chemist only learns the relative proportions in which its constituents have combined to form it. Analysis alone tells him nothing about the weight of its molecule (page 32); and until he knows this, as well as the percentage composition of a substance, he can neither tell in what numbers the atoms of its component elements occur in its molecules, nor proceed to the important work of investigating its constitution—that is to say, the relations of these various atoms to one another.

The methods of weighing molecules are, some of them physical, some of them chemical. Liebig made important contributions to the latter class, by teaching us two elegant processes, suitable for the group of compounds known as the organic bases. The first is no longer much in use, but its successor, which consists in the analysis of the compounds they form with chloride of platinum, is still frequently used, and is highly valued for its accuracy.

In concluding this brief sketch of some of Liebig's chief contributions to chemical method, all of which were characterised by simplicity, elegance, and accuracy, one cannot refrain from alluding first to the process of gas analysis, in which he employed the long-known power of alkaline solutions of pyrogallic acid to absorb oxygen, for the purpose of measuring the volume of that gas in air and other gaseous mixtures containing it, and also took advantage of the necessary use of potash in this process to combine the measurement of oxygen with that of carbonic acid gas. And, secondly, to the "Liebig's Condenser." This, however, will be already familiar to nearly every one who has visited a chemical laboratory. No single instrument has done better service to experimental chemistry than this. It is, as we all know, in daily and hourly use in every laboratory. It has become almost as essential to the work of every student of chemistry as the test-tube.

CHAPTER II.

LIEBIG AND WÖHLER.

Liebig and Wöhler—Wöhler's Early Life—His Visit to Berzelius—The Composition of Fulminic Acid and Cyanic Acid—Isomerism—Liebig and Wöhler meet—They Propose to Work Together—Researches on Oil of Bitter Almonds—The Benzoyl Theory—Study of Uric Acid and its Derivatives—The Characters of Liebig and Wöhler Contrasted—Their Mutual Esteem.

It would be impossible to tell the story of Liebig and his work without soon referring to his joint labours and life-long intimacy with Friedrich Wöhler. Their joint work and their friendship will be remembered, and will link the names of Liebig and Wöhler through all time in the mind of every student of chemistry, equally for the purity and warmth which characterised their historic alliance, and for the epoch-making results which were the outcome of their mutual endeavours.

Friedrich Wöhler was born in the neighbourhood of Frankfort on the 31st of July, 1800. Like Liebig, he showed at an early age a passion for experimenting, which is said to have been the cause of frequent neglect of his studies at the gymnasium. His scientific tastes were fostered and directed by Dr. Buch, a physician who sympathised with his inclinations, and who had himself devoted time to the study of chemistry and physics. His father, who was a citizen of some position in Frankfort, on the other hand, encouraged in him a taste for drawing, for the literature of his country, and, above all, indoctrinated him with a love of outdoor life and exercise, which was

probably in a great degree the cause of the almost constant good health that he enjoyed throughout a long and active life.

After obtaining his degree in 1823, a year later than Liebig, Wöhler determined, on the advice of Gmelin, to place himself under the direction of Berzelius, at Stockholm, who was then at the summit of his fame alike on account of his achievements in analysis and for his contributions to chemical theory; and Berzelius having been applied to, and having consented to his desire, Wöhler was soon on his way to the famous laboratory in Sweden. The following account of his reception, and of the laboratory and life at Stockholm, will be interesting to everyone who has visited a laboratory of to-day:—"With a beating heart," he says, "I stood before Berzelius's door and rang the bell. It was opened by a vigorous and portly man. This was Berzelius himself. As he led me into his laboratory I was as in a dream, doubting if I could really be in the classical place which was the object of my aspirations. . . . I was then the only one in the laboratory. . . . The laboratory consisted of two ordinary rooms, furnished in the simplest possible way; there were no furnaces or draught places, neither gas nor water supply. In one of the rooms were two common deal tables; at one of these Berzelius worked, the other was intended for me. On the walls were a few cupboards for reagents; in the middle was a mercury trough, whilst the glass blower's lamp stood on the hearth. In addition was a sink with an earthenware cistern and tap standing over a wooden tub, where the despotic Anna, the cook, had daily to clean the apparatus. In the other room were the balances, and some cupboards containing

instruments; close by was a small workshop fitted with a lathe."

"In the adjacent kitchen, in which Anna prepared the meals, was a small and seldom used furnace and a never cool sand-bath." Anna appears to have picked up the elements of nomenclature, for Wöhler tells us that he was not a little surprised on one occasion to hear Berzelius chide her for saying that some apparatus she was cleaning smelt strongly of oxymuriatic acid, saying, "Hearest thou, Anna, thou must no longer speak of oxymuriatic acid; thou must call it chlorine; that is better."

After a year at Stockholm, Wöhler's visit to Berzelius was brought to a conclusion by a short period of travel in Southern Sweden and Norway, which was partly devoted to collecting geological specimens and partly to sport. In the course of his travels he met Davy, who, like Wöhler, had a marked taste for active outdoor exercises.

This tour completed, Wöhler returned to Germany, having contracted a friendship with Berzelius which continued without interruption throughout the life of the latter, in spite of the fact that Wöhler's subsequent work with Liebig brought him into occasional conflict with the rather tightly held convictions of Berzelius on various points of chemical theory.

After his return to Germany, Wöhler accepted a post at the then recently founded trade school in Berlin, and thus obtained possession of a laboratory of his own; he stayed in Berlin about six years, and during this period, besides less important work, he succeeded for the first time in isolating aluminium by a process which has only lately been superseded: and accomplished the transformation of ammonium

cyanate, an inorganic substance, into urea, one of the most characteristic of animal products, thereby breaking down the imaginary barrier between the products of the chemical and the so-called vital forces, and opening out a new field of investigation which is still inexhausted, and seems inexhaustible.

Above all, during these years he found Liebig. The mode in which they were brought together is one of the romances of science. Whilst still a boy, Liebig's attention had been drawn to the fulminates, a class of bodies which owe their name to their violently explosive character, when watching a peripatetic dealer in odds-and-ends make fulminating silver for fire-crackers in the market-place at Darmstadt, by dissolving silver in nitric acid, and then adding a liquid which smelt of brandy—with which he also cleaned dirty coat collars for the rustics—to the product. Liebig's attention was so strongly drawn to this body, that he afterwards, as has already been mentioned, made repeated examinations and analyses of it, which were only brought to a successful issue by the work done in Gay-Lussac's laboratory in 1823–24.

At almost the same time Wöhler was occupied with his investigations of another and very dissimilar substance, cyanic acid, in the laboratory of Berzelius. When Liebig had finally satisfied himself as to the composition of fulminic acid, he was surprised to find that his results coincided with the analyses of cyanic acid made and published by Wöhler somewhat earlier.

Cyanic acid and fulminic acid were so obviously different that no one could confuse them. Such a result, therefore, seemed almost absurdly impossible.

How could the same elements combine in the same proportions to form dissimilar compounds? It was contrary to the fundamental principles of chemistry; there must be a mistake somewhere.

Confident that his own results were correct, Liebig repeated the investigation of Wöhler. But this was found to be correct too. These two compounds, so different from each other, were indeed identical in their composition.

Berzelius and some other chemists did not at first accept these concluions, in spite of the fact that some compounds were already known among inorganic bodies, which were opposed to the axiom that substances having the same qualitative and quantitative composition must exhibit the same properties. But Gay-Lussac not only accepted the results, but pointed out that the new facts might be accounted for by assuming a difference in the manner in which the constituent elements are combined in the two substances. And before very long it became impossible for any one to doubt the existence of the phenomenon of *isomerism,* as it was named by Berzelius, which was thus first recognised through the work of these two—Liebig and Wöhler. On the very threshold of their careers, these two men had made a discovery of the first order. It was inevitable that they should become rivals or friends. It was characteristic of them that they became friends. Not indeed at once, but as soon as an opportunity offered itself. They met, some time after, in the house of a common friend at Frankfort, and the acquaintance then formed soon ripened into friendship, and resulted in a frequent and free intercourse, never afterwards interrupted. Their friendship was soon cemented by

their undertaking the joint labours which have inseparably united their names in the annals of science.

The proposal that they should thus join hands came first from Wöhler, in the following characteristic letter:—

"SACROW, near POTSDAM, 8th June, 1829.

"DEAR PROFESSOR,—The contents of your last letter to Poggendorff have been communicated to me by him, and I am glad that they afford me an opportunity of resuming the correspondence which we began last winter. It must surely be some wicked demon that again and again imperceptibly brings us into collision by means of our work, and tries to make the chemical public believe that we purposely seek these apples of discord as opponents. But I think he is not going to succeed. If you are so minded, we might, for the humour of it, undertake some chemical work together, in order that the result might be made known under our joint names. Of course, you would work in Giessen and I in Berlin, when we are agreed upon the plan, and we could communicate with each other from time to time as to its progress. I leave the choice of subject entirely to you.

"I am very glad that you have also determined the identity of pyro-uric and cyanic acids. Gmelin would say:—'God be thanked, there is one acid the less.' . . .—Yours,

"WÖHLER."

Liebig expressed his joyful assent to this proposition at once, and a research upon mellitic acid—the acid of honeystone—was selected, and carried to a successful issue. Soon after publishing their results they again joined forces, at Wöhler's suggestion, to make an investigation of an acid, cyanuric acid, obtained by him from urea, in the course of which Wöhler observed the remarkable examples of molecular re-arrangement, which occur in the spontaneous transformation of the liquid cyanic acid into its isomeride, the solid cyanuric acid, and the re-conversion of the latter, on distillation, into cyanic acid once

more. It was next proposed that fulminic acid should form the subject of a new and joint attack: indeed, Liebig evidently at one time commenced operations. These, however, were soon abandoned, for he wrote on November 18th, 1830:—"The fulminic acid we will allow to remain undisturbed. Like you, I have vowed to have nothing more to do with this stuff. Some time back I wanted, in connection with our work, to decompose some fulminating silver by means of ammonium sulphide; at the moment the first drop fell into the dish, the mass exploded under my nose. I was thrown backwards, and was deaf for a fortnight, and became almost blind."

It would be impossible and unfitting in this book to enter at any length into the details of the fifteen memoirs published jointly by Wöhler and Liebig, but the results of several of their researches are of such remarkable importance, and have played so great a part in the growth of chemistry, that it is impossible not to refer to some of them.

First in importance stands their research on oil of bitter almonds, concerning which Berzelius wrote to them:—"The facts put forward by you give rise to such considerations that they may well be deemed the beginning of a new day in vegetal chemistry."

Well might Berzelius commend this splendid piece of work. It was one of the most prolific of their joint investigations. It not only gave several new and important compounds to chemistry, but it also exercised a most important influence on the growth of chemical theory, by establishing in organic chemistry the conception of what are called *compound radicles*.

When these pioneers in the new organic chemistry took the field, in 1832, oil of bitter almonds was

already known as a volatile liquid rendered familiar by its characteristic smell, and remarkable for its power when exposed to the air of absorbing oxygen and changing into a beautifully crystalline substance, then as now known as *benzoic acid*.

Armed with Liebig's method of organic analysis, the two chemists soon ascertained the true composition of these substances. They arrived at the formulæ by which, in effect, we still represent them, and made the exact relation of the one to the other easy to understand.

For the sake of those of my readers who are not acquainted with chemistry, I must here explain that, according to the conceptions of chemists, the larger masses of matter with which we are familiar may be supposed to be built up of exceedingly small and indestructible particles, called *atoms*. These atoms are so exceedingly small as to be quite beyond our powers of seeing, even when we are assisted by the most powerful microscopes. The atoms of a given chemical element are supposed to be in every way identical in their properties, including their weight. Those of different elements, however, are, on the other hand, unlike in their properties. Atoms are supposed to be endowed with the power of attracting one another in varying degrees, to form the molecules of compounds or of elements.

Chemists are in the habit of representing these atoms by means of conventional symbols. Thus the letter H represents one atom of hydrogen; O one atom of oxygen; C one atom of carbon; N one atom of nitrogen; Cl one atom of chlorine; and so on.

Compounds are represented by placing the symbols of the combined elements close to each other.

Thus, there being good reason to conclude that each molecule of water contains two atoms of hydrogen united with one atom of oxygen, we give the formula H_2O to this substance.

The theory thus briefly sketched is known as the "Atomic Theory." We owe it to an Englishman —John Dalton, of Manchester.*

When Liebig and Wöhler had worked out the formulæ of oil of bitter almonds and of benzoic acid, they found that the former might be looked upon as a compound of an atom of hydrogen with a group of atoms, or *radicle*, C_7H_5O, which we call *benzoyl*—*i.e.* as C_7H_5O, H; and that the latter might be considered to contain the same radicle united with another group or radicle, hydroxyl. This view is represented by the following formulæ, in which benzoyl, C_7H_5O, is common to both substances:—

Oil of bitter almonds	C_7H_5O, H
Benzoic acid	C_7H_5O, OH

By other experiments they discovered several more new substances which might also be regarded as compounds of benzoyl. These are given in the following table; they show well what a clearness of ideas was gained by conceiving the presence in all these compounds of a common group of atoms, which could be transferred from one compound to another, as it were, like a single atom:—

Oil of bitter almonds	C_7H_5O, H
Benzoic acid	C_7H_5O.(OH)
,, chloride	C_7H_5O, Cl
,, bromide	C_7H_5O, Br
,, iodide	C_7H_5O, I
,, cyanide	C_7H_5O, CN

* *See* "John Dalton and the Rise of Modern Chemistry" in this series.

This idea of compound radicles was not, it is true, entirely new when Liebig and Wöhler applied it in the above case, for Berzelius had, as early as in 1820, attempted to compare the constitutions of organic substances with those of inorganic compounds by the use of such a conception, whilst it was known from the researches of Gay-Lussac that cyanogen, a compound of nitrogen and carbon, acts very much like an element.

But a real appreciation of the existence of a connection between the properties of substances and the radicles they contain was mainly brought about, in the first instance, by this memorable research "upon the radicle of benzoic acid."

Well might these investigators modestly congratulate themselves on this achievement. They had discovered a true path into the almost unknown regions of organic chemistry by their "Benzoyl Theory."

But Liebig and Wöhler did not stay their hands at this point. It was known that oil of bitter almonds does not exist ready formed in the almonds themselves, and that the almonds contain a beautifully-crystalline substance—amygdalin.

In 1836 Wöhler was selected to succeed Strömeyer as Professor of Chemistry at Göttingen. The choice lay between Liebig and Wöhler on this occasion. As soon as he was ready for fresh work, he wrote to Liebig:—"I am like a hen which has laid an egg and straightway sets up a great cackling. I have this morning found how bitter oil of almonds containing prussic acid may be obtained from amygdalin, and would propose that we jointly undertake the further investigation of the matter, as it is intimately related to the benzoyl

research." A few days later he wrote that he had made a remarkable discovery in relation to amygdalin. Bitter almond oil can be obtained from amygdalin and also from almonds, and it occurred to him that the oil might be produced from the amygdalin by an action similar to that of a ferment. Experiments showed that this hypothesis was well borne out by the facts, for he found that an emulsion of sweet almonds which contain no amygdalin, causes the formation of the oil and of prussic acid, when it acts upon amygdalin. From this they inferred the presence both in bitter and sweet almonds of a "kind of soluble ferment," to which they gave the name *emulsin*. Nor was this all. From their analyses of amygdalin, and of oil of bitter almonds and prussic acid, they satisfied themselves that in the transformation of the former something besides the oil and prussic acid must be produced; something had been missed. They soon discovered this complementary product. It was sugar. And thus they made known to chemistry, for the first time, a member of another new and most important and interesting group of substances—viz. the *glucosides*, and at the same time made an important contribution to our knowledge of *ferments*.

The joint work of Liebig and Wöhler was continued till 1838, when their grand investigation of uric acid was published. Their experiments soon showed that the interest of uric acid to the chemist is hardly inferior to that which this substance excites in the physiologist. The readiness with which it takes part in chemical change is such that, in a single research, they added no less than sixteen new and most remarkable substances to the list of organic compounds, concerning which it is notable that in the course of

nearly half a century only one of them disappeared from the science. On this occasion, as on that of their work on oil of bitter almonds, their memoir was not only remarkable for the number of the new substances which it introduced into chemistry, it was again distinguished both by the masterly interpretation of their results, in which they again made use of their conception of organic radicles, and by the prescience, which enabled them to foresee the direction in which organic chemistry was about to advance. From these researches, they said:—"The philosophy of chemistry must draw the conclusion that the synthesis of all organic compounds which are not organised must be looked upon not merely as probable, but as certain of ultimate achievement. Sugar, salicin, morphine will be artificially prepared. As yet, we are ignorant of the road by which this end will be reached, since the proximate constituents required for building up these substances are not yet known to us; but these the progress of science cannot fail to reveal." This was the last great research undertaken by these two friends. Liebig soon afterwards turned his attention to the problems of agricultural and physiological chemistry, whilst Wöhler thereafter devoted himself chiefly to inorganic chemistry.

It is natural, nay inevitable, that reference should be made to the human side of the friendship between these two men, whose names are so entwined with one another and with the history of chemistry. Hofmann, the pupil of Liebig and the editor of their correspondence, has left us a picture of the men in which each figure stands clearly before us—"Liebig, fiery and rash, seizing a new idea with enthusiasm,

readily giving free rein to his imagination, tenacious of his opinions, yet open to the recognition of error, sincerely grateful, indeed, to those who made him conscious of it. Wöhler, calm and deliberate, approaching a new problem with temperate consideration securely guarded against over-hasty conclusions; but both equally inspired by the same invariable love of truth. Liebig, irritable, easily offended, hot-tempered, hardly master of his emotions, which often found vent in bitter words that involved him in long and painful quarrels. Wöhler, unimpassioned, even under the most malignant provocation, disarming the bitterest opponent by the sobriety of his speech, the sworn foe of quarrels and dissension, yet both animated by the same unerring sense of right." Can we wonder that between two such natures, so different and yet so complementary, there should ripen a friendship that they might count among the best harvests of their lives. The following letter from Wöhler, on the occasion of one of Liebig's fits of annoyance, indicates the manner in which Wöhler's influence was exerted on Liebig:—

"GÖTTINGEN March 9th, 1843.

"To make war against Marchand, or, indeed, against anybody else, brings no contentment with it and is of little use to science. . . . Imagine that it is the year 1900, when we are both dissolved into carbonic acid, water, and ammonia, and our ashes, it may be, are part of the bones of some dog that has despoiled our graves. Who cares then whether we have lived in peace or anger; who thinks then of thy polemics, of the sacrifice of thy health and peace of mind for science? Nobody. But thy good ideas, the new facts which thou hast discovered—these, sifted from all that is immaterial, will be known and remembered to all time. But how comes it that I should advise the lion to eat sugar?"

Nor was this the only occasion on which Wöhler sought to moderate the occasional intemperance, under provocation, of his friend. Thus in a letter of March 3rd, 1834, on the occasion of another dispute, he warns his friend that he may be right and may be doing a service to knowledge, but that he embitters his life and ruins his health for nothing.

On the other hand, it was to Liebig and Giessen that Wöhler turned when, in 1832, he lost his young wife, and it was by working in Liebig's company that he sought for consolation and forgetfulness after his loss. On which occasion he wrote after his return to Cassel—"I am here back again in my darkened solitude . . . How happy was I that we could work together face to face."

And again on another occasion he wrote:—"The days which I spend with Liebig slip by like hours, and I count them among my happiest."

As it has sometimes been suggested that Wöhler received something less than his fair share of credit for the work done with Liebig, the following extract from Liebig's autobiographic sketch may fitly close this brief account of their labours and friendship.

Speaking of his work at Giessen, he says, "I had the great good fortune from the commencement of my work at Giessen to gain a friend of similar tastes and similar aims, with whom, after so many years, I am still knit in the bonds of warmest affection.

"While in me the predominating inclination was to seek out the points of resemblance in the behaviour of bodies or their compounds, he possessed an unparalleled faculty of perceiving their differences. Acuteness of observation was combined in him with an artistic dexterity, and an ingeniousness in discovering new

means and methods of research or analysis, such as few men possess.

"The achievement of our joint work upon uric acid and oil of bitter almonds has frequently been praised; it was his work. I cannot sufficiently highly estimate the advantage which the association with Wöhler brought to me in the attainment of my own as well as of our mutual aims, for by that association were united the peculiarities of two schools—the good that was in each became effective by co-operation. Without envy and without jealousy, hand-in-hand, we plodded our way; when the one needed help, the other was ready. Some idea of this relationship will be obtained if I mention that many of our smaller pieces of work which bear our joint names were done by one alone; they were charming little gifts which one presented to the other."

Wöhler, on the other hand, wrote as follows: "We two, Liebig and I, have dissimilar kinds of talent; each, when in concert, strengthens the other. No one recognises this more fully than Liebig himself, and no one does me greater justice for my share of our common work than he."

Assuredly neither of these two undervalued the services rendered to him by the other. Their friendship was as nobly unselfish as it was useful.

CHAPTER III.

CHEMICAL DISCOVERIES.

Practical Importance of some of his Discoveries—Method of Making Cyanide of Potassium—Chloroform and Chloral—Experiments on Ammonium Thiocyanate—Conflict with Gerhardt—How Liebig Missed Discovering Bromine—Nature of Acids—Theory of the Polybasic Acids—Hydrogen Theory of Acids—Distinction of Equivalent from Molecular Weights—Progress of Radicle Theory—Ethyl Theory—Compound Radicles.

In chemistry, as in the arts and manufactures, there are certain substances which form, as it were, the raw material from which others are fabricated. Thus salt is the raw material for makers of soda and soap. Sometimes the starting-point, so to speak, of a group of manufactures is not a natural product like salt, but one that must itself be prepared on the large scale from other substances. This is the case with yellow prussiate of potash.

The manufacture of yellow prussiate of potash on the large scale had long been practised, but the nature of the changes by which it is formed was first made clear by the experiments of Liebig, with the result that his discovery led to improvements in the process, which cheapened it and greatly extended its uses. One of its most important applications is in the making of potassium cyanide. Liebig devised an easy, safe, and inexpensive process for preparing the latter salt by melting a mixture of the yellow prussiate of potash with carbonate of potash. The salt was thus

made available for many new purposes. The following are some striking examples:—

Potassium cyanide dissolves several silver salts which are insoluble in water alone. This property was very useful in the early days of photography; photographers made use of it for removing the unaltered silver compounds from their negatives. Cheap cyanide of potassium thus helped on the development of this useful and interesting art.

Again, it is found by electro-platers that silver can best be deposited from a solution of silver cyanide in potassium cyanide. Hence a cheap method of making the cyanide went far to render electroplating, practically speaking, possible; and finally the low price at which it can now be produced enables miners to use a solution of it for extracting finely divided gold from the rocks, even when the gold occurs, as it often does, only to the extent of less than one ounce to the ton. Thus, without counting the importance of this substance in pure chemistry, the progress of three highly important branches of industry have been immensely promoted by this one simple discovery.

Nor does this example of the practical value of investigations which at first sight may seem to be of purely chemical interest by any means stand alone. One more instance of even greater importance must be mentioned.

In 1832 Liebig's experiments on the action of chlorine with alcohol resulted in the discovery of a substance of absolutely immeasurable value to man—viz. of chloroform, the anæsthetic—and of a second substance, chloral, also of great, though not of such supreme, importance as the former. Liebig described how to prepare these compounds, and gave

their properties,* and he observed the remarkable fact that chloral when it acts with potash yields chloroform. Fifteen years afterwards, as we all know, chloroform was first employed as an anæsthetic by Simpson, of Edinburgh. But it was not till twenty years later that Oscar Liebreich, inspired by Liebig's observation that chloral yields chloroform under the influence of alkalies, formed the happy idea of studying its physiological action, with the hope that the small amount of alkali in the blood would be sufficient to effect the transformation of chloral into chloroform (and formic acid), with the result that he discovered the interesting and unexpected physiological qualities of that substance.

It would be impossible within the space which this part of Liebig's work can claim in this book to give even a superficial account of the numerous substances discovered by him and described in the three hundred and eighteen papers that bear his name; but those who have even an elementary knowledge of chemistry will recognise their importance when, to mention only a few, I say that they include such bodies as ferrocyanic acid, aldehyde, meta-aldehyde, thialdine, carbothialdine, and creatinine and sarcosine, the decomposition products of creatine from flesh.

Neither is it possible to do justice to the almost endless variety of his miscellaneous observations, to the long list of organic substances the composition of which either he or his pupils determined, to the numerous plant-ash analyses that were made in his laboratory, to the processes with which he endowed physiology, to his analyses of German mineral waters,

* Dumas first correctly determined the composition of these substances.

or to his contributions to technology, such as his processes for silvering mirrors, and for making unfermented bread. These by themselves might have made a reputation for a not undistinguished man of science.

There are, however, other contributions to chemistry by Liebig of equal rank with those which have been already discussed. For example, his investigation of the compounds derived from ammonium sulphocyanate, and the conflicts with Gerhardt and Laurent to which it gave rise. And, again, the part he played in the discussions of the great theoretical questions which followed the establishment of the Atomic Theory and agitated chemists during the period of his greatest activity as a student of pure chemistry.

The first of these is all the more interesting because it affords at once an excellent illustration of Liebig's method of work, and also an example of the advantages that often flow from those conflicts between experimentally determined facts on the one hand, and theoretical interpretations on the other; or between old established views and new conceptions, which so often agitate the followers of an active branch of experimental science, and whose significance is usually so little understood, or rather so completely misunderstood, by the world at large, and by the cynics in particular.

It is true enough that such struggles are sometimes conducted in too vehement a manner, but this is not always nor, indeed, very often the case. Besides, is there not, after all, a great element of nobility in every one of these struggles, in which both sides aim equally at truth, and both are equally free, in spite of occasional excess of zeal, from

all petty and sordid desires for personal advantage? The struggle in these contests is not to decide who shall gain most advantage from the result, but who shall do most in the service of humanity.

When examining the effect of heat and certain reagents on ammonium thiocyanate, Liebig found that, instead of breaking up into simpler substances, as he expected, it gave rise to a series of new products of ever-increasing complexity. His experiments led him to give the following formulæ to the new products:—

1. Melamine	. . .	$C_3\,N_3\,(NH_2)_3$
2. Ammeline	. . .	$C_3\,N_3\,(NH_2)_2\,(OH)$
3. Ammelide	. . .	$(C_3\,N_3)_2\,(NH_2)_3\,(OH)_3$
4. Cyanuric Acid	. .	$C_3\,N_3\,(OH)_3$

Gerhardt, looking at the formulæ given in the above table from a purely theoretical point of view, quickly perceived a certain want of symmetry in the relations of the compounds represented, and pointed out that the substance which might have been expected to occupy the third place in the table was a substance with the formula $(C_3N_3)\,(NH_2)\,(OH)_2$, melanurenic acid, in which case the series would be written as follows:—

Melamine	. . . .	$C_3\,N_3\,(NH_2)_3$
Ammeline	. . . .	$C_3\,N_3\,(NH_2)_2\,(OH)$
Melanurenic Acid	. . .	$C_3\,N_3\,(NH_2)\,(OH)_2$
Cyanuric Acid	. . .	$C_3\,N_3\,(OH)_3$

And he did not hesitate, supported only by theory, to declare that Liebig's analyses must be wrong. Liebig at once entered a solemn protest against this use of theory unsupported by experiment, declared it to be in opposition to all sound principles of scientific inquiry, and smashed the critical part of

the case of his antagonist by producing from urea, jointly with Wöhler, the compound which Gerhardt had only imagined to exist, and showing that its properties were different from those of ammelide.

But whilst Liebig's reproaches to Gerhardt were doubtless justified, as regards his method of criticism, the importance of attacking such subjects from the theoretical side is well shown by the subsequent production of Gerhardt's hypothetical melanurenic acid. Gerhardt's fault lay, not in his theorising, but in not subjecting his hypothesis to the test of rigorous experiment before attempting to discredit the experimental results obtained by another.

On the occasion of another discussion Liebig again drew attention to the importance of never trusting an untested hypothesis, by telling a story against himself. Early in his career, speculating without experimenting cost him and Germany the discovery of bromine.

"No greater misfortune," he said, "can befall a chemist than being unable to disengage himself from preconceived ideas, and yielding to the bias of his mind to account for all phenomena not agreeing with his conceptions by explanations not founded on experiment. . . I know a chemist who, while at Kreuznach many years ago, undertook an investigation of the mother-liquor from the salt works. He found iodine in it; he observed, moreover, that the iodide of starch turned of a fiery yellow by standing overnight. The phenomenon struck him; he procured a large quantity of the mother-liquor, saturated it with chlorine, and obtained by distillation a considerable amount of a liquid colouring starch yellow, and possessing the external properties of chloride of iodine, but differing in many of its reactions from the latter

compound. He explained, however, every discrepancy most satisfactorily to himself; he contrived for himself a theory on it.

"Several months later he received the splendid paper of M. Balard, and, on the very same day, he was in a condition to publish a series of experiments on the behaviour of bromine with iron, platinum and carbon; for Balard's bromine stood in his laboratory, labelled liquid chloride of iodine."

One of Liebig's most important contributions to chemical theory has already been brought forward in connection with his joint research with Wöhler upon oil of bitter almonds. This has justly been termed one of the pillars of the theory of compound radicles. It was largely owing to Liebig's influence, also, that the new ideas regarding the nature of acids first brought forward by our countryman, Sir Humphry Davy, in 1809, were, after a period of neglect, once more prominently brought under the notice of chemists.

According to the ideas of the earlier chemists, all acids and all salts must contain oxygen.

The first blow at this conception concerning acids and salts was struck when Davy, after his famous investigation of hydrochloric acid and chlorine, renounced the hypothetical "*murium*," whose oxide was supposed to exist in hydrochloric acid, and boldly represented this acid as a combination of the element chlorine with hydrogen.

Having observed that oxide of iodine only becomes an acid after it is dissolved in water, Davy and Dulong subsequently went further, and concluded that hydrogen, and not oxygen, is the true acidifying element; that hydrogen, in fact, is the essential constituent of acids.

The opinions of Davy and Dulong on this subject were opposed by Berzelius, as they appeared not to be reconcilable with the electro-chemical theory in which he had combined and developed the dualistic hypothesis of Lavoisier and the electro-chemical conception of our great countryman Davy, and, consequently, the views of Davy and Dulong lost ground, until Daniell's studies in electrolysis led to new ideas of the electro-chemical constitution of acids and salts.

The final return to the views of Davy and Dulong was greatly helped on by the papers in which Liebig brought forward his theory of the polybasic acids.

In the earlier years of the nineteenth century most chemists held views on the relations of acids and alkalies which practically involved the assumption that the molecules of all acids are of equal value in their power of neutralising alkalies, until Graham, in 1833, published his investigations of the phosphoric acids, and showed that when phosphoric oxide dissolves in water it can generate three distinct acids, with very different powers of neutralising alkalies.

Liebig, in 1837, paid a visit to England, when he formed a high opinion of Graham, of whom he says, "Graham . . . modest and without pretence, makes wonderful discoveries." About the same time he visited Paris and Dumas, and from a letter written to Wöhler, after his return, it would appear that he then began to form new views on the constitution of the acids. In the same year he published, jointly with Dumas, a paper in which they proposed that the accepted formula for citric acid should be trebled, thus making this a tribasic acid.* Liebig afterwards

* *Note.*—The neutralising power of a molecule of a dibasic acid is twice as great as that of a molecule of a monobasic acid, and so on.

returned to the subject, and described experiments with a whole host of acids and their salts, in which the existence of monobasic, dibasic, and tribasic organic acids was clearly indicated, and in the course of his work he soon saw that the hydrogen theory of acids was both probable and convenient. Acids he defined as particular compounds of hydrogen, in which the latter can be replaced by metals.

It was at one time objected that Davy's theory involved the necessity of admitting the existence of a host of radicles which had not been, and in most cases still have not been, isolated; but this, as Liebig pointed out, was equally true of the earlier view. Very few of the acid anhydrides which were supposed to enter into the formation of salts had at that time been discovered, whilst in addition the experimental evidence which could then be brought forward was all against the existence of such hypothetical elements as the "murium," which was supposed to be a constituent of the hypothetical acid anhydride of the chlorides. To support the earlier, and at that time more orthodox, view it was necessary not only to admit the existence of non-isolated radicles, but also to invent non-existent elements from which to construct them. Hypothesis had to be supported by hypothesis. It was natural, therefore, that Liebig, who was ever forward in denouncing such a use of theory in science, should be amongst the foremost of those who supported the new conceptions which were more securely based on the proved truths of chemical science.

The chemists of to-day—nay, even comparatively young students of the subject—are accustomed to

distinguish with precision between the chemical conceptions of the molecule, the atom, and the equivalent. When Graham and Liebig worked on phosphoric and the organic acids, far less distinct notions prevailed on the subject, and especially on the proper employment of the terms "equivalent" and "atom."

The term *atom* was, as has previously been explained, introduced into chemistry by John Dalton to designate certain very small indivisible particles of which matter is supposed to be composed. Dalton considered that chemical compounds were formed by the uniting or approximating of atoms of different elements, and that the atoms of each element were exactly alike in all their properties, including their weight. He published certain tables in which he professed to give the relative weights of the atoms. These numbers were calculated from the relative proportions of the elements found in their compounds. Wollaston, in 1808, and afterwards Davy and Gay-Lussac, denied that Dalton's *atomic weights* were really the relative weights of the atoms, and Wollaston proposed for them the name *chemical equivalents*. The use of this term was not, however, always confined to its original purpose; it came to be extended to compound substances as well as to the elements. Thus it happened that when the word was applied to elements, it was apt to be used very much in the early sense of the term atomic weight; whilst as applied to compounds, it often signified more nearly what we now define as the molecular weight. The clearing-up of the confusion thus created was initiated by Liebig, to whom we owe the first precise expressions of the distinction between the

equivalent weights and the molecular weights* of substances.

One of the most interesting discussions in which Liebig assisted related to the constitution and relations of alcohol and ether.

According to Liebig, the relation of these organic compounds to the inorganic substances is very simple and intelligible. They contain a compound radicle, ethyl (C_2H_5), alcohol being its hydroxide and ether its oxide. Ethyl may be compared to the element potassium, and its compounds to those of potassium. Thus—

Ether $(C_2H_5)_2O$	corresponds to the oxide K_2O.		
Alcohol $(C_2H_5)OH$	,,	,,	hydroxide KHO.
Ethyl chloride $(C_2H_5)Cl$	,,	,,	chloride KCl.

He did not, it is true, at once arrive at the "ethyl theory" exactly as stated above. Still, the modern view is in all its essentials founded solely on the views of Liebig, according to which organic chemistry was defined as the "chemistry of organic radicles," whilst these radicles were compared with the elements, and their combinations with the corresponding inorganic substances. "Organic chemistry," said Liebig and Dumas, "possesses its own elements, which sometimes play the part of chlorine or oxygen, sometimes that of a metal. Cyanogen, amidogen, benzoyl, and the radicles of ammonium compounds, of fats, and of alcohol and its derivatives, constitute the true elements of organic nature. . . . ."

* Liebig used the word atom in his writings where we use molecule, for the exact distinction between atoms and molecules was accomplished later by his successors.

CHAPTER IV.

LIEBIG AND DUMAS.

Dumas's Early Life—Dumas at Geneva—His Meeting with Humboldt—Paris—Substitution—Conflict with Dualism—Liebig accepts Substitution Theory—Chemistry of Vinegar-making—Aldehyde.

THE two chemists who were chiefly associated with Liebig in directing the course of organic chemistry during the third and fourth decades of the nineteenth century were Wöhler, in Germany, and Dumas, in France. Wöhler and Liebig almost from the time of their first meeting were, as we have seen, closely knit in friendship, and for many years were intimately associated in the prosecution of common studies.

The relations of Liebig and Dumas were not always equally harmonious; these two sometimes found themselves in opposite camps. Both of them were men who could take as well as give hard knocks, however, and hence their frequent scientific encounters never prevented either of them from appreciating the high qualities of the other, and on several occasions they worked in unison for a common object. When Liebig dedicated a German edition of his "Familiar Letters on Chemistry" to Dumas, in 1851, with characteristic open-heartedness he addressed the following note to his old opponent:—

"MY DEAR DUMAS,—It was by a strange coincidence that for more than a quarter of a century our labours in the cause

of the science to which our lives have been devoted were prosecuted in the same direction.

"If the roads by which we endeavoured to attain the goal were often different, in the proximity of that goal we always met in order to shake hands with each other.

"Not only your country, but the whole scientific world, acknowledges the range, the depth, and the importance of your researches and discoveries, but no one knows better than myself the obstacles which your genius had to surmount in order to achieve those inestimable conquests which, in a measure, constitute the foundation of modern science. Though contending with difficulties of every kind, you never descended into the arena without leaving it as conqueror.

"Permit me, in recognition of the services which you have rendered to science and to mankind at large, to dedicate to you this little work, in which I have ventured to sketch for an enlarged circle of readers the onward movement of scientific and applied chemistry, to which you have so much contributed. Your approbation would be the highest reward I could possibly hope for. "LIEBIG.

"Giessen, 1851."

Whilst Dumas, not less generous than his old friend and opponent, when referring to their labours in organic chemistry in his commemorative speech on Pelouze, said:—

"Into this as yet uncultivated domain we had plunged, Liebig and I, with most living ardour. The number of organic substances, nowadays immense, was even then very considerable. Their study, however, if we except the group of bodies selected by Chevreul for his researches, had not as yet elicited results of any great importance. The nature of most compounds was unknown; their differences, their analogies, their connections had still to be unveiled."

"To find our way through these unexplored territories, we had neither compass nor guides, neither method nor laws. Each of us had been led to form

ideas and to elaborate views peculiar to himself, which he defended with warmth and even with passion but without any feeling of envy or jealousy. The discoveries to be made appeared to us without limit, and each was satisfied with his harvest. What we both had at heart was to stake the ground and open roads, nor have I any doubt that in reading my papers Liebig felt the same pleasure which the perusal of his afforded me. If a new step had been taken, it was of little moment whether it had been made by the one or by the other, since it served us both on the road to truth."

Jean Baptiste André Dumas was born at Alais, in the department of the Gard, July 14th, 1800, and, like Liebig, became apprentice to an apothecary; but not finding much opportunity for scientific progress in his position he soon migrated to Geneva, travelling there on foot. Here he found both employment and means of education, and very quickly developed in a surprising degree his talent for experimental investigation.

The turning-point in the career of Liebig was, as we have seen, probably his meeting with Alexander von Humboldt and his introduction by the latter to Gay-Lussac in 1823. Singularly enough, it was a day spent with the great traveller which induced Dumas to turn his face to Paris at a moment when there was much to induce him to settle in Geneva.

The story of their meeting was told by Dumas himself to Hofmann, and it illustrates so well the fascination that Humboldt could exercise on a youth of genius that, though it is rather foreign to the purpose of this book, it must not be omitted:—

"One day," said Dumas, "when I was in my study completing some drawings at the microscope, and, it

must be added, rather negligently attired to enable me to move more freely, some one mounted the stairs, stopped on my landing, and gently knocked at the door. 'Come in,' said I, without looking up from my work. On turning round I was surprised to find myself face to face with a gentleman in a bright blue coat with metal buttons, a white waistcoat, nankeen breeches, and top boots. This costume, which might have been the fashion under the Directory, was then quite out of date. The wearer of it, his head somewhat bent, his eyes deep set but keen, advanced with a pleasant smile; saying, 'Monsieur Dumas?' 'The same, sir; but excuse me.' 'Don't disturb yourself. I am M. de Humboldt, and did not wish to pass through Geneva without having had the pleasure of seeing you.' Throwing on my coat, I hastily reiterated my apologies. I had only one chair; my visitor was pleased to accept it, whilst I resumed my elevated perch on the drawing stool. Baron Humboldt had read the paper published by M. Prevost and myself, on blood, and was anxious to see the preparations I had by me. His wish was soon gratified. 'I am going to the Congress at Verona,' said he, 'and I intend to spend some days at Geneva, to see old friends and make new ones, and more especially to become acquainted with young people who are beginning their career. Will you act as my cicerone? I warn you, however, that my rambles begin early and end late. Now, could you be at my disposal, say, from six in the morning till midnight?' This proposal, which was, of course, accepted with alacrity, proved to me a source of unexpected pleasure. Baron Humboldt was fond of talking: he passed from one subject to another without stopping. He obviously

liked being listened to, and there was no fear of his being interrupted by a young man who, for the first time, heard Laplace, Berthollet, Gay-Lussac, Arago, Thenard, Cuvier, and many others of the Parisian celebrities, spoken of with familiarity. I listened with a strange delight; new horizons began to dawn upon me. . ."

"At the end of a few days Baron Humboldt left Geneva. After his departure the town seemed empty to me. I felt as if spellbound. . . I had been more especially impressed with what he told me of Parisian life, of the happy collaboration of men of science, and of the unlimited facilities which the French capital offered to young men wishing to devote themselves to scientific pursuits. I began to think that Paris was the only place where, under the auspices of the leaders of physical and chemical science, with whom, I had no doubt, I should soon become acquainted, I might hope to find the advice and assistance which would enable me to carry out the labours over which I had been pondering for some time. My mind was made up: I must go to Paris."

In the year 1823, therefore, Dumas went to Paris. Before long he obtained the appointment of Répétiteur de Chimie to Thénard's course of Lectures in the École Polytechnique. From this time his attention was directed to the study of chemical phenomena.

Such were the first steps of Liebig's great colleague and rival, Dumas.

If Liebig had so frequently the happiness of witnessing the triumph of his ideas, and had so often the gratification of observing the development of organic chemistry proceed along the lines which he himself had laid down, on the other hand, he occasionally

found it necessary at the end of a discussion to accept the conclusions of others or to remain opposed to the truth. At such times he never hesitated; once let him be convinced of the justness of the views of an opponent, and he was among the very foremost to welcome an advance, or the discovery of a new road, into the unknown. This quality of his mind is well illustrated by his attitude to the theory of substitution, which in the hands of Dumas and his successors effected the final overthrow of the dualistic view of chemical phenomena, and greatly modified the early form of the theory of compound radicles itself.

The dualistic idea was introduced into chemistry by Lavoisier; it reached its highest development in the hands of Wöhler's master, Berzelius. According to Lavoisier, however great may be the complexity of a compound we may always detect in it evidence of two constituent parts. These may be either simple, as in the oxides—*e.g.* oxide of calcium, or quicklime, which contains a non-metal, oxygen, united with a metal, calcium—or compound, as in the salts, which he regarded as produced by the union of an oxide of a metal, on the one hand, with an acid (usually the oxide of a non-metal) on the other. Early in the century Berzelius, by his electro-chemical theory, had offered an explanation of dualistic combination which was consistent with the knowledge of electrolysis then possessed by chemists, and thus re-established the position of dualism in the science at a moment when it seemed to be seriously threatened by the new facts which had lately been brought to light by the investigations of Davy.

Berzelius started with the assumption that the

atoms are themselves electric, and possess at least two poles whose quantities of electricity are in most cases unequal. Thus the elements could be classed as positive and negative according to which electricity prevailed. Chemical combination, according to this theory, consists in the attraction of the dissimilar poles of the atoms, and consequently in the neutralising of the two electricities. As, however, these were not always equal in amount, the compound produced was itself frequently electric, and therefore capable of entering into further combinations. Thus, according to Berzelius, each compound consists of two different parts, as suggested by Lavoisier, which attract each other in consequence of their different states of electrification.

In accordance with these ideas Berzelius represented the composition of salts by such formulæ as the following, in which the electro-chemical form of the dualistic view of chemical combination will at once be recognised :—

Barium sulphate	$\overset{+}{\mathrm{Ba\,O}}\ \overset{-}{\mathrm{SO_3}}$
Calcium sulphate	$\overset{+}{\mathrm{Ca\,O}}\ \overset{-}{\mathrm{SO_3}}$
Copper sulphate	$\overset{+}{\mathrm{Cu\,O}}\ \overset{-}{\mathrm{SO_3}}$

When organic chemistry began to develop in the hands of Liebig, Wöhler, and the French chemists, Berzelius directed his attention especially to the task of bringing the radicle theory of organic chemistry into agreement with the fundamental ideas of his electro-chemical form of dualism. According to him compound radicles were unalterable groups, and he thought that organic chemistry, like inorganic chemistry, must accept oxygen as the

supreme ruler among the elements, and give it the place which it had held in the mineral world since the time of Lavoisier.

But, meanwhile, new departures were imminent which were fated to work great changes in the radicle theory almost before it had come to maturity.

One evening, at a *soirée* at the Tuileries, during the reign of Charles X., the pleasure of the entertainment was seriously marred by the fact that the wax-candles emitted very unpleasant, irritating fumes, and were remarkable for the smokiness of their flames. The investigation of the cause of their peculiar behaviour was entrusted to Dumas. The irritating fumes were found to be hydrochloric acid, and Dumas had no difficulty in discovering that they were due to the candles having been made from wax which had been bleached by chlorine. This circumstance led him to investigate the action of chlorine upon organic bodies. He soon found that, when chlorine acts on compounds containing hydrogen, the hydrogen may be removed and replaced by an equivalent quantity of chlorine. This observation was not, it is true, a new one; Gay-Lussac, Faraday, and Liebig and Wöhler had all observed that hydrochloric acid is emitted, and chlorine fixed by organic bodies. But Dumas first systematically examined this kind of action, laid down the rules which it follows, and, jointly with Laurent, who first perceived their significance, developed their consequences. How was it possible, they asked, to continue to accept the electro-chemical hypothesis, with its rigidly appointed and opposite functions for electro-positive and electro-negative elements, when it was thus shown to be possible for an electro-negative atom, like that

of chlorine, to replace an electro-positive atom of hydrogen?

Dumas' earliest results were soon splendidly confirmed by the discovery of trichloracetic acid. By suitably treating acetic acid with chlorine Dumas produced from it another acid, which differed from the first by containing three atoms of chlorine in place of three atoms of hydrogen in every molecule. "It is chlorinated vinegar," said Dumas; "but what is very remarkable, at least for those who refuse to find in chlorine a body capable of replacing hydrogen in the precise and complete sense of the word, this chlorinated vinegar is just as much an acid as common vinegar itself. Its acid power is not changed. It saturates the same quantity of alkali as before, and saturates it equally well, and the salts to which it gives rise exhibit, when compared with acetates, resemblances full of interest and generality."

"Here, then, is a new organic acid, containing a very considerable quantity of chlorine, and exhibiting none of the reactions of chlorine; its hydrogen has disappeared, and has been replaced by chlorine, and yet this remarkable substitution has produced only a slight change in its properties, all its essential characters remaining unaltered."

"If its internal properties are modified, this modification becomes apparent only when, through the intervention of a new force, the molecule itself is destroyed and transformed into new products . . . It is evident that, in confining myself to this system of ideas dictated by facts, I have not in any way taken into consideration the electro-chemical theories on which Berzelius has generally based the idea

predominating in the opinions which this illustrious chemist has endeavoured to enforce."

"But do these electro-chemical ideas, this special polarity attributed to the molecules of elementary bodies, rest upon facts so evident that it is necessary to erect them into articles of faith? Or, if they must be regarded as hypotheses, have they the power of lending themselves to facts, of explaining and foreseeing them with so complete a certainty as to have afforded important assistance in chemical researches? It must plainly be allowed that this is not the case."

At first Berzelius received these new ideas with something like disdain. Such assertions, put forward as they were at first by Laurent, a beginner, were still without authority; they appeared to him unworthy of serious refutation. But when Dumas came into the field, Berzelius energetically defended his opinions. In this he was at first vigorously supported by Liebig, who admitted, indeed, the fact of substitution, but protested against the wide conclusions Dumas drew from his results, and met him with an ironical rejoinder in the form of a letter,* purporting to come from S. C. H. Windler, which ran much as follows:—

"The last great discovery from Paris shows that it has been found possible to replace in acetate of manganese first the atoms of hydrogen by chlorine, then the oxygen, then the manganese, and at last even the carbon, so that a body was formed which contained only chlorine, but retained still the properties of the original substance." He continued, after alluding to the method of bleaching cotton goods by chlorine: "I understand that there are already in the London shops stuffs made of chlorine thread much approved in

* This letter was written by Wöhler, and published by Liebig.

the hospitals and preferred to all others for night-caps, under-garments, etc."

However, further facts, such as the re-converting of chloracetic acid into acetic acid by the action of nascent hydrogen, and especially the production of the chlorine and bromine derivatives of aniline in his own laboratory by his pupil, Hofmann, before long convinced Liebig that the character of a chemical substance does not depend so much as he had supposed on the nature of its constituent atoms, but very largely also on the manner in which these atoms are arranged, and he declared that the interpretations proposed by Dumas, of the facts relating to substitution, appeared to him to afford the explanation of a great number of phenomena in organic chemistry.

Some years afterwards, at a dinner given by the French chemists to chemical visitors to the Exhibition of 1867, Liebig made his defeat on this occasion the source of a happy retort to Dumas, who had asked him why of late years he had devoted himself exclusively to agricultural chemistry. "I have withdrawn from organic chemistry," said Liebig, "for with the theory of substitution as a foundation, the edifice of chemical science may be built up by workmen: masters are no longer needed."

Of course, this must be taken in the spirit of the after-dinner speech; but the reply shows the completeness with which Liebig extended his admiration to a great achievement, even when it had not been reached without some little opposition from himself and some damage to his own ideas.

To conclude this long list of some of Liebig's chief contributions to pure chemistry, which, as will presently be seen, only represents a part of the field

covered by his work, we must now glance at his inquiry into the chemistry of the change of alcohol into vinegar. This subject closes most suitably an account of Liebig's labours in chemistry, because it leads us by a natural transition to a chemico-biological inquiry in which he took a prominent part—viz. to the question of the nature of fermentation.

It has long been a familiar fact that moderately dilute solutions of alcohol, such as wine or beer, when exposed to the air, become, under certain conditions, converted into vinegar, but the exact nature of the chemical changes involved for long remained unexplained.

Liebig soon dispelled the obscurity in which this subject had so long remained, by showing that the change from alcohol to vinegar (acetic acid) takes place in two stages, viz. that when alcohol is submitted to the action of an oxidising agent—to the action of a substance which readily parts with oxygen, that is to say—it first loses hydrogen, which is removed in the form of water, by which change a substance called aldehyde is produced; and, secondly, that the aldehyde takes up oxygen to form acetic acid. Frequently, of course, these two changes proceed simultaneously, so that they become indistinguishable, but by suitable methods Liebig secured the intermediate product, and by doing so presented to us a new substance, which has given its name to a class—the aldehydes—and still remains in the minds of chemists as the aldehyde *par excellence*. One useful quality of aldehyde, which Liebig was the first to observe and employ, must be mentioned. If one adds a few drops of aldehyde to a flask or

test-tube containing a solution of lunar caustic—nitrate of silver—rendered slightly ammoniacal by an addition of ammonia, and then gently warms the mixture, at once the glass becomes coated with a film of silver, reflecting more perfectly than an ordinary mercury mirror. This reaction affords both a useful test for aldehyde and a ready and simple method of preparing mirrors which is often useful, and makes it possible to avoid the danger to health and life attached to the older process in which mercury is used.

CHAPTER V.

FERMENTATION.

Liebig's Theory of Fermentation—Supposed Influence of Oxygen—Difference between Fermentation and Decay—Fermentation of Alcohol—"Quick Vinegar Process"—The Vitalistic Theory revived by Pasteur—Discussion between Liebig and Pasteur—Lactic Ferment—Vinegar Plant—Enzymes.

FERMENTATION.—The word fermentation is derived from *fervere*, "to boil," and may be supposed to owe its origin to the effervescence which occurs when saccharine liquids are left to themselves in contact with air, or placed in contact with a ferment such as yeast. Naturally, such a wonder-working process as fermentation has always attracted the interest of the observant, and numerous indeed have been the conjectures hazarded in the various attempts which have been made to fathom its mystery. It was impossible, however, as will soon be seen, that much real progress should be made by the earlier thinkers on the subject, for, only when the microscope had been brought to a state of considerable efficiency, and when, at least, a good start had been made in organic chemistry, was it possible to get any light on this absorbing yet bewildering subject.

To many the word fermentation still implies very little; to most it probably merely connotes the useful process by which the sugar of malt or of grapes is converted into beer or wine, or by which flour and water are made to yield bread; or again, the pernicious

change by which badly made preserves are apt to lose their attractiveness of flavour and become both distasteful and unwholesome. It is, therefore, necessary to explain that these are only a few instances from a very large class of diverse changes, all of them brought about by substances or organisms known to chemists and biologists as the "*ferments*."

According to Liebig, fermentation is to be considered an essentially chemical phenomenon; according to his opponents, fermentive changes depend, in many cases at least, on the life-processes of certain minute organisms. To the latter, therefore, fermentation, in these cases at any rate, is not a chemical, but rather a biological, phenomenon.

It is one of the distinguishing features of a "ferment" that a very little of it goes a long way. A minute fragment of rennet is sufficient to cause the curdling of a relatively large quantity of milk. A single yeast cell may bring about the fermentation of the largest vessel of grape juice, and presently the grape juice will be converted into wine. If but the point of a needle touch a liquid in which the ferment of anthrax has been cultivated, a prick from that needle will be sufficient to communicate anthrax to any animal susceptible to the disease.

These, and others like them, are the phenomena which must be explained by a theory of fermentation.

An early attempt to explain these fermentive changes chemically was that of Berzelius. Lavoisier having shown that sugar is split up by fermentation into alcohol and carbonic acid gas, Berzelius suggested that the action of the yeast is "catalytic" —that is, that the ferment brings about the de-

composition of the sugar, by mere contact, much as platinum black causes hydrogen peroxide to decompose into water and oxygen, or as manganese dioxide causes chlorate of potassium to give up its oxygen at a lower temperature than is required for the decomposition of the salt by heat alone. For some time this so-called explanation of the phenomenon seems to have been accepted as satisfactory, but inasmuch as the nature of a catalytic action was not itself understood, it did not in reality throw much, if any, new light on the subject.

Liebig pointed out that universal experience teaches that all organised bodies after death suffer a change, in consequence of which their remains gradually vanish. From the smallest twig to the largest tree all vegetables disappear after a few years, whilst animal matters once deprived of life and if exposed to the air are dissipated in a much shorter time, leaving only their mineral matter behind them. This great process, which requires for its progress air and moisture, results finally in the converting of their carbon into carbonic acid gas, of their hydrogen into water, of their nitrogen into ammonia, and of their sulphur into sulphuric acid. They are then in the forms in which they can serve as food for new generations of plants and animals. Those parts which were derived from the air are returned to the air, and the mineral parts which were taken from the earth are returned once more to the soil. The death followed by the dissolution of one generation is the source of life for a succeeding generation. The atoms of carbon and hydrogen, which yesterday formed part of the brain or muscle of an Englishman, may to-morrow contribute to the material parts of a

Persian or a native of Japan. The processes which bring about these resolutions of organic matter into the very same simple bodies from which they themselves were formerly produced, belong to the class which we are considering—to the fermentations. They require, in order that they may occur, moisture, and at the earlier stages the presence of air; afterwards, in many cases, fermentations can proceed even if air be excluded.

These are the facts on which Liebig's theory of the ferments was founded. Reasoning upon them, he said: "It is obvious that by the contact of these organic compounds with the oxygen of the air, a process begins, in the course of which their constituents suffer a total change in their properties. This change is the result of a change in their composition. Before contact with oxygen, their constituents are arranged together without action on each other. By the oxygen the state of rest or equilibrium of the attractions which keep the elements together has been disturbed in a particle of the substance, and, as a consequence of this disturbance, a separation or new arrangement of the elements has been brought about.

"The continuance of these processes, even when the oxygen, the original exciting cause of them, no longer acts, shows most clearly that the state of decomposition which has been produced among the elements of a particle of the mass exerted an influence on the other particles which have not been in contact with the oxygen of the air; for not only the first particles, but, by degrees, all the rest undergo *the same change.*

"All those processes of decomposition," he con-

tinues, "which begin in a part of an organic substance from the application of an external cause, and which spread through the whole mass, with or without the co-operation of that cause, have been called processes of *putrefaction*. A putrescible substance, therefore, is distinguished from one not putrescible, because the former, without other conditions than a certain temperature, and the presence of water (after exposure, although transient, to the atmosphere), are resolved into a series of new products, while the latter, if unmixed, do not, under the same circumstances, undergo any change."

The number of substances, however, which are thus putrescible is few, though they are widely diffused. They are all of them highly complex, containing nitrogen and sulphur—such things as albumin, fibrin, and gelatin, for example.

On the other hand, a great number of substances, such as sugar, starch, and the organic acids which are found in the juices of plants, are not putrescible if pure; if exposed to air and moisture, they do not undergo any perceptible change; a solution of sugar, for example, when exposed, dries up and deposits crystals which retain their original properties.

If, however, some sugar, or sugar of milk, &c., be dissolved in water, and if the solution be added to a portion of a putrescible substance already in a putrid condition, these substances will then be fermented, that is to say, will undergo a change.

Substances like sugar and starch were termed by Liebig the fermentescible substances. The process of their decomposition under the influence of the putrescible substances, according to him, is what we call fermentation. And it will be perceived by this

time that the putrescent matters which can thus induce the decomposition of the "fermentescible" substances are the *ferments*.

If one examines the juices of vegetables, or fluids of animal origin, one finds present in them always, in greater or less quantity, the instable compounds of the first class (Liebig's ferments), as well as substances of the second class. This fact explains, according to Liebig's hypothesis, why all such juices undergo fermentation after contact with air, in the course of which they are ultimately reduced to substances of simpler composition than before.

The products of a fermentation are, as we know, commonly of more simple composition than the substances from which they are derived. Thus, grape sugar, which has the formula $C_6H_{12}O_6$, when fermented yields chiefly alcohol, C_2H_6O, and carbonic acid gas. Sugar of milk may similarly be made to yield a simpler compound—lactic acid. In order that such changes as these may occur, it seemed obvious that the atoms of the substance fermented must be set in motion, since, in order to rearrange themselves, it is plain that they must move. From this Liebig concluded that the power of the ferments over fermentable substances depends on a certain state or condition of their atoms, and, further, that this state must be one in which the complex molecules of the ferments undergo resolution into simpler molecules. Thus, the action of a ferment on a fermentable compound, seemed to Liebig not unlike that of heat on an organic compound. In virtue of the motions of the atoms of its own molecules, it disturbs the equilibrium previously existing among the atoms of the molecules of the fermented matter,

and new and simpler combinations result. This analogy he found to be confirmed by the influence of temperature on fermentation. If an organic substance be submitted to a given temperature certain definite products are obtained, which may, however, be altered if a higher temperature be employed. Similarly, the products of fermentations are markedly influenced by the temperatures at which the fermentations occur.

But, it will be asked, what is it that starts these internal movements, which changes a merely putrescible substance into an actually putrescent body or ferment?

Liebig was of opinion that the oxygen of the air was the first cause of the breaking-up of the nitrogenous substances.* The immediate and most energetic cause of all the alterations and transformations which organic molecules undergo is, he says, "the chemical action of oxygen." That is why exposure to the air is a necessary preliminary condition for the commencement of a fermentive change.

Fermentation, according to Liebig, is only a consequence of the commencement of a process of decay, in which oxygen plays a part, and it continues till the fermenting substance has resolved itself into a series of new products which undergo no further change, unless as the result of further causes of alteration.

But although a state of rest may thus be reached in regard to the attractions among the atoms of the newly-formed substances, yet this condition of equilibrium would not exist with regard to their

* Familiar letters.

attraction for oxygen. The chemical action of oxygen would only cease when the capacity of the elements to combine with oxygen was exhausted. Fermentation, therefore, represents only the first stage of the resolution of complex molecules into simpler ones. The process is completed by *decay*, which he defined as "a process of combustion taking place at common temperatures, in which the products of the fermentation and putrefaction of plants and of animal bodies combine gradually with the oxygen of the atmosphere," producing carbonic acid, water, and ammonia.

There are many compounds which, by themselves, are deficient in the property of absorbing oxygen. Alcohol, for example, only combines with oxygen at a comparatively high temperature. For such things contact with a substance itself undergoing change was held by Liebig to be the chief condition of decay. They then behave, he said, as if they were a part of the decaying material, and their oxidation is effected. He offered as an instance the German "quick vinegar process," in which alcohol, in the form of wine or diluted brandy, is allowed to flow slowly over shavings of wood, packed in casks through which a slight current of air also circulates. The alcohol, in spite of its want of attraction for oxygen is, under these circumstances, quickly converted into vinegar. This would not, however, occur if pure spirit were passed over a non-putrescible material, and at the commencement of the process it is usual to add to the spirit a small quantity of one of those substances which are capable, unassisted, of undergoing decay—such as beer-wort, honey, vinegar—then, after the lapse of a short time, the surface of the shavings passes,

according to this view, into a state of oxidation, and from that time there is no further need of the co-operation of added decaying matter.

Such was Liebig's explanation of the phenomena of fermentation and decay.

But not even the authority of a Liebig will for long protect a wide-reaching hypothesis, such as this, from criticism, and soon its great rival, the vitalistic theory of fermentation, was revived by Louis Pasteur, who brought to its support observations and new facts of a most startling character.

The starting-point of the vitalistic theory of fermentation is to be found in the observation made in 1680, by Leuwenhoeck, when examining beer yeast with the microscope, that this substance consists of small globules, which are spherical or globular in form, but whose nature he was not able to determine. A century and a half later the observations of Cagniard de Latour, and soon afterwards those of Schwann, at Jena, and Kützing, at Berlin, showed that yeast consists of a mass of organic globules capable of reproducing themselves by buds; these globules seemed to them to belong to the vegetable kingdom, and not to be simply organic chemical compounds, as had till then been supposed to be the case. They concluded that probably the converting of sugar into alchohol and carbon dioxode was an effect of their vegetative processes. This view of the action of yeast cells was not, however, widely accepted till it had the support of Pasteur's experiments, and not universally, even then, until after a controversy of the keenest kind.

Liebig did not deny the organised nature of yeast, nor its power of multiplication by budding, but he

was of opinion that the living cells are always accompanied, also, by dead cells, and that it was the molecular motions of the decaying matter of these dead cells which was communicated to the sugar and brought about its decomposition by fermentation.

To put this idea to the test of experiment, Pasteur, who was not one whit behind Liebig himself in his conviction that experiment is the final court of appeal on all scientific questions, sowed almost imponderable portions of fresh yeast cells in solutions of pure sugar, to which he had added small quantities of such mineral salts as are necessary for their growth, with the result that the cells thus sown multiplied, and the sugar fermented as before. From this result he concluded that the process mainly took place between the sugar and a ferment germ, which owed its life and development to the nutritive matter he supplied to it. The most important of these nutritive substances was the sugar. That fermentation, in short, is simply a phenomenon of nutrition, in which the organism assimilates one part of the fermentable matter, using it for its growth and for the production of new individuals, and converts the rest into the well-known products of the change. This attempt at a crucial experiment by means of the fermentation of pure sugar, in association with mineral salts, does not, by itself, as a close examination shows, really finally overthrow Liebig's hypothesis, for it is manifestly reasonable to suppose, first, that even the small amount of ferment taken, however carefully purified, must have been accompanied by a certain amount of what Liebig called putrescible material; and, secondly, that the growth and life of the yeast would be soon accompanied by the death of some part of it. This

would supply the putrescible matter even if it were absent in the first instance.

It is scarcely surprising, therefore, that Liebig was not for some time convinced of the dependence of fermentation on the existence of these organisms by the facts brought forward by his opponent: and he made merry over these minute beings which feed on sugar, and secrete alcohol, whose appearance simultaneously with the fermenting of the sugar, had been held to support the idea that fermentation is a result of their vital processes. To him the presence of animalculæ in putrefying matter appeared to be of the nature of an accident, their number, when large, being the result of the fact that these organisms find in such matters the most favourable conditions for their nutrition and development, their presence being often very beneficial, since it resulted in a more rapid oxidation of the material concerned. Fortunately for science, Pasteur did not confine himself, as his predecessors had done, to the investigation of a single case of fermentation. He carried his researches into other fields, and thereby enriched science with many new facts, some of which are of incalculable importance, in consequence of the light they have thrown on the nature of several of the most virulent diseases by which animals and men are afflicted, and because they stimulated other observers to make equally productive exertions in many new directions.

When Liebig's earliest opinions were formed on the subject of fermentation, only the fermentation of sugar by yeast had received any considerable attention on its biological side, but subsequently Pasteur extended the range of his investigations, and was able to connect the existence of other organisms with

other fermentive changes. Thus he found that the lactic fermentation, in which sugar of milk produces lactic acid, is associated with the growth of a grey substance which may be easily overlooked, partly because the amount produced is usually relatively small, partly on account of the difficulty of distinguishing it from the other materials present.

The grey matter in question, when separated from other substances, appears, as seen under the microscope, to be formed of small globules or points smaller than those of beer yeast. Pasteur showed a fairly complete analogy between the known facts relating to these two fermentations, and proved further, that whilst in the presence of both ferments both fermentations can proceed under suitable conditions, lactic acid is never produced in normal alcoholic fermentation.

Vinegar, as has previously been explained (p. 71), is produced by exposing wine or diluted brandy to the air ; oxygen is absorbed by the alcohol present in these liquids, and the alcohol is thereby converted into acetic acid. Pure alcohol will not undergo this oxidation; it is necessary, as will be remembered, to add a little beer-wort, meat-juice, or some such putrescible body, in order that "acetic fermentation" shall set in. It is necessary, in fact, to have a "ferment," and Liebig, as we know, believed the ferment to be nitrogenous matter in a state of change. Pasteur, on the other hand, attributed the acetous fermentation to a plant, which had long been known under the name of "flower of vinegar," a little fungus which floats on the surface of wine during the process which transforms it into vinegar. The yeast plant sometimes forms a hardly visible veil over the surface of the liquid, at

others it exists as a wrinkled film, very thin and unctuous to the touch. This little organism, which can only exist if it be plentifully supplied with its proper aliments and at a moderate temperature, possesses, Pasteur contended, the power of condensing considerable quantities of oxygen from the air and of fixing this gas on alcohol. It is one of those so-called spontaneous productions which, like moulds, almost always make their appearance on liquids suitable for their growth; they or their germs appear to exist everywhere around us, so that if one wants them it is generally only necessary to expose a suitable nutrient liquid—in this case a mixture of wine and vinegar will do well—in a warm place, and they will soon make their appearance.

Pasteur showed that, owing to the sensitiveness of the vinegar plant, it is destroyed by a temperature of a little above 60° C., and that wine heated to that temperature, even if afterwards exposed to filtered air, refuses to become vinegar. This temperature, he argued, must have left intact the albuminous and nitrogenous substances in the wine, and hence these cannot be regarded as the source of the fermentation; in short, the ferment in this case must be the living vinegar plant.

Such experiments as those which have been quoted, and perhaps still more the constant connection which has been shown by Pasteur and others to exist between certain fermentive diseases and definite organisms, have now for some time past satisfied nearly everyone that fermentation, including in that term also putrefaction and decay, is, as Pasteur has so vehemently insisted, in many cases connected with the existence of organisms, and that each kind of fermentation is

dependent on the growth and development of a particular organism. To this very considerable extent Pasteur has fixed the vitalistic theory on so secure a base, that even Liebig practically admitted its truth in his later writings. And yet, in spite of the success of the vitalistic theory, it cannot be said that this theory has overthrown its rival. Although the connection of many fermentations with the life of definite organisms has been clearly proved, there remain a number of changes, which may also be described as fermentations, the occurrence of which, it seems almost certain, does not depend upon such organisms. And, besides, though it is clear that in many cases fermentation depends on the presence of micro-organisms, we do not yet know how these organisms act. We do not know whether they live on the fermentable matter and excrete the products of the fermentation, or whether the microbes produce soluble ferments, such as will be presently mentioned, which afterwards bring about the fermenting of the fermentable substances in some manner more or less like that which Liebig suggested. As regards the first alternative, it may be pointed out that the amount of the products of a fermentation is usually very great, in proportion to the mass of the organisms concerned and to the time they are in action. On the other hand, as regards the second alternative, though attempts to isolate soluble ferments from the organisms have not often been successful, yet it has been done. Hence, *a priori*, there is nothing improbable in the second suggestion, as the past failure to isolate these substances may well have been due to want of knowledge and experience. The difficulty is always to begin.

This brings us again to the further important fact, that besides the organised ferments whose existence was established by Pasteur and others, there are yet other ferments of quite a different nature; such are the active agent (emulsin) in the change by which the amygdalin of bitter almonds yields sugar, prussic acid, and oil of bitter almonds (*see* p. 35), and the ptyalin of saliva, by which starch may be transformed into a sugar. These latter ferments are called *enzymes*. They are believed to be unorganised chemical substances which result from the activity of living cells. The changes which they bring about are analogous to those induced by the organised ferments themselves. Like the latter, they appear to be independent of any change in the agent which produces them, and in both cases great effects are produced by what seem to us, at first sight, very trivial causes. Whilst Liebig was wrong in denying the existence of any connection between the organisms and the fermentations, it must be admitted, on the other hand, that his opponents have not yet succeeded in explaining their mode of action, so that it is not impossible that some modification of the theory of Liebig may yet be found useful. The rival theories of fermentation which have here been discussed have jointly and severally done a splendid work, by stimulating and guiding workers in this field of science, from which rich crops have already been gathered, and which seems to promise results in the future, such as the earlier workers would scarce have dared to dream of when they began their labours.

It now seems possible that the difference between an organised and an unorganised ferment may be this—that the active agent of the organised ferment

is one which only acts within the cells in which it is formed, and which has not yet, with one or two exceptions, been separated from the cells which contain it; whilst the unorganised ferments are those which can act outside the cells which produce them.

Whatever may be the final fate of Liebig's theory of the ferments, I fear it must be said, after nearly half a century of interesting, active work, that it has not yet been replaced by a really general and satisfactory hypothesis.

CHAPTER VI.

CHEMISTRY OF AGRICULTURE.

Commemorative Addresses—Components of Plants—Relations of Plants and Animals—Davy's Lectures—Boussingault's Laboratory—The Humus Theory—Opinions in Germany and England—Overthrow of Humus Theory—Evidence that Air is Source of Carbon of Vegetables—Plants Source of Carbon for Animals—The Real Use of Humus—Sources of Components of Plants Other than Carbon—Origin of Nitrogen of Plants—Mineral Theory of Manures—Liebig's Experiments in Agriculture—Errors Made in Applying the Mineral Manures, and how they were Corrected—"Ground Absorption" of Soils—Object of Liebig's Practical Work in Husbandry—"The Natural Laws of Husbandry"—Deterioration of Land in Western Countries; How to Avoid it—Liebig's Influence on Education of Agriculturists.

SPEAKING generally, the first twenty years of Liebig's life, after his return from Paris, were devoted to pure chemistry. The next period was distinguished by his remarkable contributions to chemistry as applied to agriculture and physiology. Perhaps some idea of the range and depth of his work over the whole field of pure and applied chemistry may be gained from the fact that, on his death, in 1873, it was felt to be impossible that any one man could sufficiently comprehend all the subjects he had advanced and his share in their advancement, and that, therefore, not one, but three of his colleagues, all men of great eminence, were charged with the duty of delivering commemorative addresses, each one of them undertaking that part of the great master's work with which

he was himself most familiar from his own life-work. (*See* p. 9.)

Liebig's first writings on agricultural chemistry and on fermentation were presented in 1840 * to the Members of the British Association, as part of a Report upon the Present State of Organic Chemistry, in fulfilment of a task which had been imposed upon him by its chemical section at a previous meeting. From the work which he then began, in response to the duty thus laid upon him, he may be said never to have withdrawn his hand. The subject was worthy of the worker. "Perfect agriculture," as he says, in his preface to this book, "is the true foundation of all trade and industry, it is the foundation of the riches of states. But a rational system of agriculture cannot be formed without the application of scientific principles, for such a system must be based on an exact acquaintance with the means of nutrition of vegetables, and with the influence of soils and actions of manure upon them. This knowledge we must seek from chemistry, which teaches the mode of investigating the composition, and of studying the character of the different substances from which plants derive their nourishment. . ."

These were the convictions which impelled Liebig to bring his unique and now highly-trained faculties to bear on the task of creating a science of agriculture, and to enter a field which had lain fallow since the time of Davy, who was the first chemist to occupy himself with the study of the application of chemical principles to the growth of vegetables and to organic processes.

* Liebig's "Chemistry in its Applications to Agriculture and Physiology." Published in Brunswick, 1840. English Edition Edited by Dr. Lyon Playfair, London, 1840.

When Liebig began this, his second great work, he was not far from the zenith of his career. No one was better trained than he in the methods of organic analysis, which he had brought to perfection and applied to their purpose, as Wöhler says, with almost pedantic exactness. He was surrounded, too, by a number of pupils eager and well qualified to aid him in his undertaking; among them at this moment was Lyon Playfair, to whom was entrusted the duty—well carried out—of editing Liebig's first book, for publication in England.

Liebig's first book on chemistry, in its applications to agriculture and vegetable physiology, quickly passed through a number of editions. Twenty-two years later, after he had studied in minute detail the various questions involved in this subject, and taken part in numerous discussions with agriculturists and others, at home and abroad, he published an embodiment of his researches in "The Natural Laws of Husbandry," a work which has been truly described by Hofmann as the first perfect construction of the philosophy of agriculture which had ever appeared up to that date. This work was originally issued in two parts. The second part was published in an English translation under the above title.

Liebig spared no pains in order to qualify himself on the technical side for this new undertaking. Besides studying the practice of husbandry at home, he paid a visit to Great Britain, and made a journey through the agricultural districts of England and Scotland, in order that he might acquaint himself personally with the various practices of different districts in farming, in butter-making in cheese-making, and so on.

Side by side with his labours on behalf of agriculture, Liebig undertook another and equally searching investigation into the applications of chemistry to physiology and pathology. It was a happy inspiration which led him to combine in his researches these two lines of work, each so important to the welfare of mankind, each so closely bearing upon the other. The study of agriculture, consists largely in the study of the science of manuring. Liebig's strict investigation of the processes of nutrition in the animal organism, and of the origin of animal excrements, enabled him in the end to understand, better than those who preceded him, the cause of the beneficial effects of these excrements on the growth of vegetables, and enabled him to trace among the multitudinous phenomena of life processes, a few simple yet fundamental laws for the guidance of practical farmers.

Interesting as it would be to follow the details of Liebig's agricultural investigations during the years of his chief activity in this direction, it is impossible to attempt it. Were we to do so, we should see him now at his desk, now working in his laboratory, now guiding the older students in their share of his studies; at another time we should have to follow him to the factory, where his mineral manures were prepared; then go with him to his experimental plot to watch their effects; or accompany him to some gathering of agriculturists, scientific or otherwise, to join in discussions, which were often warm and always animated, on the agricultural topic of the moment. In no part of all these varied duties did Liebig fail to take his full share. The work of the porter in the manure factory, that of the

labourer on the farm, and of the student at his analysis was all done under his close guidance, and was as fully the outcome of his inspiration as that part which was done with his own hands and head alone. But, delightful as it would be thus closely to watch the activities of such a man, all that can be even attempted is to try to gain a clear view of the ideas which animated Liebig's work at its inception, of the main features of the labours he undertook, of the difficulties he met and overcame, and, finally, of the position to which agricultural science had been raised when these labours were brought to a conclusion.

For the benefit of the uninitiated, it must here be pointed out, that there is a beautiful connection between the organic and the inorganic kingdoms of nature. It is inorganic matter mainly which affords food to plants, and they, on the other hand, yield the means of subsistence to animals.

The conditions necessary for animal and vegetable nutrition are essentially different. An animal requires for its development, and for the sustenance of its vital functions, a certain class of substances which can be generated only by certain organic beings possessed of life—viz. by the plants. Although many animals are entirely carnivorous, yet their nourishment is ultimately derived from plants, for the animals on which the carnivora feed receive their nutriment from vegetable matter. Plants, on the other hand, find new nutritive material only in inorganic substances. Hence, one great end of vegetable life is to generate matter adapted for nourishing the animals out of inorganic substances which are not fitted for that purpose.

These are almost the very words with which Liebig opened his book on "Chemistry in its Application to Vegetable Physiology and Agriculture." He devoted the first part of this book to an examination of the matters which supply nutriment to plants, and of the changes which these undergo in the living organism.

The second part of the book deals with fermentation, putrefaction, and decay; but this department of Liebig's work has already come before us.

All parts of all plants contain carbon and hydrogen: these are among the constituents of all their organs; they are absolutely essential to their existence.

The substances of which the greater part of vegetables are composed also contain oxygen; the larger part of these substances are compounds in which carbon is present, together with hydrogen and oxygen in the proper proportions for forming water. Sugar, starch, gum, and the materials of which the outer walls of plants are formed are all of this character; such substances are called by chemists *carbohydrates*. Other oxidised substances also occur, in which the proportion of oxygen is greater; many of these are sour bodies, such as the vegetable acids. Wax, resin, and the fixed oils usually contain these three elements, but in their case oxygen is either absent or is present in smaller proportion than that which is found in the carbohydrates.

Besides this, the juices of plants contain small quantities of mineral matter which may be detected in their ashes, whilst nitrogen is present, especially in the seeds. The proportion of nitrogen is small, but it is rarely altogether absent from any part of a plant; it is a component of such substances as the gluten of

wheat, vegetable albumen, and of the alkaloids, such as quinine and strychnine. Finally, plants also contain a little sulphur and phosphorus.

From these facts it follows that the nourishment of a plant must contain carbon, nitrogen, phosphorus, and sulphur in forms suitable for their assimilation. Water and mineral matter must also be supplied to it.

From what sources are these obtained? Which of them, if any, come from the air, which from the soil? Are the sources of supply exhaustible or inexhaustible, practically speaking? Does nature provide modes of replenishing the supplies of all or any of them? How far do the lives of animals and the arrangements of mankind supplement or oppose themselves to such replenishment? If the latter tend to exhaust the supply of any constituent or constituents, how can these arrangements be modified so as to prevent this, or, better, so as to produce enrichment in place of exhaustion? These are some of the great questions for the agriculturist and the agricultural chemist.

How can we educate the agriculturist and the man of science, so that they shall face these problems with an open mind, and co-operate in attempting to solve these and similar questions? This is one of the great problems of technical education; considering the vital importance of agriculture, it is perhaps *the great question* of technical education.

It was said above that Liebig first, after Davy, attempted to construct a theory of agricultural chemistry; but it should be mentioned that very shortly after the commencement of Davy's work, De Saussure published his "Chemical Researches on Vegetation," and that from 1834 Boussingault also worked, with devotion, at agricultural chemistry.

The results these two investigators published are frequently referred to by Liebig in the course of his writings, and there is no doubt at all that the data they supplied contributed very largely to form the basis of Liebig's historic generalisations.

The foundations of the chemistry of agriculture were of necessity laid somewhat late, for, from what has been said, it will be seen that it was impossible to gain any true conceptions on the subject until an accurate knowledge of the composition of air and water had been attained.

When Liebig commenced the studies which ended in the overthrow of the humus theory, the composition of the air, of water, and of carbonic acid gas were known. The importance of minerals to plants was admitted, and the advantage of adding them in the form of marl to manure had been suggested, more than a century earlier, but their mode of action was still under discussion. Thaer, the agriculturist, regarded them as non-essential, or at best useful as a kind of stimulant, whilst De Saussure and Davy were opposed to this view. Priestley had observed that plants possess the faculty of purifying air that had been vitiated by the breathing of animals or by combustion, and it had been discovered that the bubbles of gas which growing plants emit when plunged under water consist chiefly of oxygen.

Ingenhousz had shown that these phenomena demand the presence of sunlight, and Sennebier that the oxygen emitted by plants finds its source in the carbon dioxide absorbed from the air. De Saussure, who worked at the end of the last century, had, by experiments that were approximately quantitative, shown that in sunlight plants increase the quantity of

carbon, hydrogen, and oxygen in their tissues, and that this is done at the expense of carbon dioxide and water; by an important experiment he had also proved that the increase of carbon and of the components of water correspond pretty closely to what we know to be the proportions of them in the carbohydrates. (*See* p. 85.) He had also recognised the usefulness of minerals, that they must come from the soil, and had called attention to the probability that the origin of the mineral constituents of the animals was to be found in these incombustible substances extracted by plants from the earth. In regard to the already much debated point of the origin of the nitrogen of plants, De Saussure was of opinion that nitrogen was rather given to the air than taken from it, and that the sources of nitrogen were probably the nitrogenous substances in the soil and the ammonia of the air, which is largely brought down to the earth by rain. On the whole, though he does not seem to have distinctly disowned the humus theory (*see* below), he regarded the air as the main source of the carbon, hydrogen, and oxygen of plants, and the earth as valuable in affording supplies of mineral matter and nitrogenous substances.

The humus theory.—Owing to the fact that virgin soils are often particularly well suited for the cultivation of plants, and that these virgin soils have been found to be very rich in vegetable mould, or *humus*, it had come to be supposed, prior to the date of Liebig's earliest writings on agricultural chemistry, by many chemists and agriculturists, that this vegetable mould was the source of the fertility of these soils. By an extension of this idea many vegetable physiologists ascribed the fertility of all

soils to its presence, and even regarded it as the chief nutriment of the plants; it was supposed by them that the humus was extracted from the soil by the roots of the growing plants.

In Germany this view was very widely accepted. Even De Saussure is said by Ernst von Meyer, in his history of chemistry, not to have kept himself quite clear of this error, though he realised much better than many others that the air, more than the earth, was the source of the carbon and hydrogen of plants. In England, as Dr. Playfair pointed out long ago, this idea of the function of humus was by no means so unreservedly accepted when Liebig wrote; but one rises from a careful examination of what Davy, Daubeny, Thomson, and Brande wrote during the early part of the century, very far from convinced that these authorities unreservedly accepted the alternative hypothesis suggested by Priestley's observations on the purifying of the air by plants. In fact, even in England, a clear perception of the source of the carbon of plants was still to seek. Thus Brande, writing as late as 1836, still doubted whether plants could efficiently remove the carbonic acid gas from the air, and considered that the carbon of manure, reduced by putrefaction to a soluble state, was an important source of supply.

Liebig, in dealing with this subject, went straight to the root of the matter. Humus is of very variable composition, and is only soluble in water when freshly precipitated; it becomes quite insoluble after drying, and after it has been exposed to a freezing temperature. Both the heat of summer and the cold of winter, therefore, render this substance insoluble. This is confirmed by the fact that water does not

dissolve anything more than a mere trace of organic matter from good garden soil, and that the decayed wood of various trees, which largely consists of humus-like matter, is also practically insoluble.

But, in order that humus shall be absorbed by plants, it must first become capable of entering into solution. It follows, that humus, as it occurs in the earth, cannot serve to nourish plants.

This was not denied by physiologists. And in order to overcome the difficulty, they assumed that the lime and the alkalis found in the ashes of vegetables render the humus soluble, and so fit it for being assimilated by the roots of plants.

It is true that alkalis and alkaline earths (*e.g.* lime) do exist in different soils in sufficient quantities to form compounds with humus, and that humus is rendered soluble in water by their presence. One kilogramme of lime, for example, will combine with 10·9 kilos. of humic acid.

To test the validity of the hypothesis based upon these facts, Liebig made various calculations, the results of which clearly showed that the amount of humus which could possibly be obtained by the plants on a given area is far too small to account for the vegetables actually produced, even after making all sorts of impossible assumptions in favour of the theory

He supported the facts brought to light by his calculations by others.

Thus, he showed that the proportion of carbon produced on one acre of cultivated land from such various crops as fir-wood, pine-wood, beech-wood, beet-root, rye and hay, is remarkably constant under dissimilar conditions of cultivation.

Also, that carbon may be removed to some extent

from a forest or meadow in the form of wood or hay, and that, in spite of this, the soil will become richer, not poorer, in carbon. And finally, he clinched the argument by pointing out that since it is universally admitted that humus is only produced by the decay of plants, no primitive humus can have existed for the first plants, for plants must have preceded the humus.

These were the facts and arguments by which, once and for all, Liebig rendered the humus theory utterly untenable by any reasonable human being.

Anarchy, however, is no more acceptable in science than in society, and Liebig was the last man to be satisfied with the mere overthrow of an erroneous theory. He was bound by his nature to ask himself the question—Whence, then, did the first vegetables obtain their carbon ?

Having thus demolished the humus theory, Liebig proceeded to investigate its rival. Ever since Priestley's observations, in 1771, on the influence of plants on the air, vegetable physiologists had, more especially in England, shown an inclination to suppose that part at least of the carbon of plants is derived from the carbonic acid gas of the air, and as early as 1807 Thomson had observed that if plants are deprived of this gas they droop their leaves, which then refuse to fulfil their functions. De Saussure and others had abundantly confirmed the English observer's conclusions, and it was beginning to be generally admitted in this country that probably a large quantity of carbon is obtained by plants from this source, and that oxygen is simultaneously supplied to the air. But further progress was retarded, partly by the apparent existence of an alternative

source of supply in the humus of the soil, and partly by the discovery by Ingenhousz that, although in sunlight plants absorb carbonic acid gas and give out oxygen, in the dark, on the contrary, they rather injure the air than improve it, as they then absorb oxygen, and in some cases emit carbonic acid gas. The oxygen given out in the light was, moreover, even said to correspond to that absorbed in the dark.

Naturally, under these circumstances it seemed doubtful whether, on the whole, very much carbon was really derived from the air by plants, and the uncertainty on this point helped to maintain the position of the humus theory until, as we have seen, Liebig demolished it in 1840. But one of the most remarkable facts about the composition of the air is this, that in spite of the vast consumption of oxygen by animals, by combustion, and in decay, the proportion of oxygen in pure air is, at all times and in all climates, practically uniform. The analysis of air always shows that one hundred volumes of air contain twenty-one volumes of oxygen. And yet, vast as is the store of oxygen in the air, so great is the consumption thereof that the supply cannot be considered to be inexhaustible. Liebig, indeed, made a calculation which showed that at the rate at which we now use it the air would become quite unfit for us in less than 300,000 years, owing to loss of oxygen alone, if the supply were not renewed. How is it possible, he asked, that with so vast a consumption of oxygen the supply should remain so nearly constant; that the analysis of air taken from jars buried in Pompeii nearly 2,000 years ago should, under these circumstances, present so nearly the same composition as a sample taken from the air—it may be yesterday.

Again, it had been found in Liebig's day, from the analyses of De Saussure, that the proportion of carbonic acid gas in the air was actually not above one part in a thousand.* What then has become of the vast quantities of carbonic acid gas that have been poured into the air during countless ages by the breathing of animals, by the process of combustion, and in the decay of organic substances—quantities so great that, according to the above-mentioned calculation, in a period of 100,000 years they must have caused an accumulation of this gas amounting to several parts by volume in every one hundred volumes of air?

We have even reason to believe that the proportion of carbonic acid may once have been greater than now, and yet, in spite of the enormous volumes of this gas which are daily poured into the air, there is to-day only a trace of it present. It is evident, said Liebig, that the invariable proportions of carbonic acid gas and oxygen in the atmosphere and the mere trace of the former that is found there, in spite of the enormous and never-ending supplies, must be the result of some cause which prevents the increase of carbonic acid gas by removing that which is constantly forming; and that there must be some means of replacing the oxygen removed from the air by the processes of combustion and putrefaction, as well as by the respiration of animals.

He found the answer to these questions in the data supplied by his predecessors, Priestley, Sennebier, and De Saussure. Both these causes are united in the processes of vegetable life.

* The more refined methods of modern analysis place the proportion still lower.

The carbon of plants cannot come from the earth; it must be derived from the atmosphere. The only form of carbon which exists in the atmosphere is carbonic acid gas, or oxide of carbon; it follows that this must be the source of the carbon of vegetables.

But, further, the analysis of plants shows that the greater part of their constituents are composed of carbon, together with hydrogen and oxygen, in the proportions in which they form water; from this it follows that the oxygen required for the nutrition of a plant can be mainly supplied by the water which it assimilates, since hydrogen enters into the food of plants almost entirely in this form, and if this be so the oxygen of the carbonic acid gas is quite unnecessary for plants, and must escape into the air in the gaseous form. As carbonic acid gas contains its own volume of oxygen, the atmosphere must thus receive for every volume of carbonic acid gas decomposed by the plants an equal volume of oxygen.

This view was not only in accordance with the facts already mentioned, but De Saussure had shown that the gain in weight of a growing plant is greater than can be accounted for by the carbon assimilated, which agrees with the supposition that the elements of water are assimilated at the same time as the carbon.

"The life of plants," said Liebig, "is closely connected with that of animals, in a most simple manner, and for a wise and sublime purpose.

"The presence of a rich and luxuriant vegetation may be conceived without the concurrence of animal life, but the existence of animals is undoubtedly dependent upon the life and development of plants.

"Plants not only afford the means of nutrition for the growth and continuance of animal organisation,

but they likewise furnish that which is essential for the support of the important vital process of respiration; for, besides separating all noxious matters from the atmosphere, they afford an inexhaustible supply of pure oxygen, and they thus make up to the air the loss constantly sustained by it. Animals, on the other hand, expire carbonic acid gas, whilst plants inspire it, and thus the composition of the atmosphere, the medium in which both live, is maintained constantly unchanged."

It was only necessary to prove that the amount of carbonic acid gas in the air, at most only one-tenth per cent., is sufficient to supply the wants of the whole vegetation on the surface of the earth, in order to finally establish the doctrine so clearly enunciated in the above paragraphs. This was not difficult, for the total mass of carbon in the air is immense, although its proportion is small relatively to that of the other constituents. But is it possible that it can be absorbed sufficiently rapidly from so dilute a mixture?

This seemed to Liebig quite likely to be the case when he considered the wide area which the leaves offer to the gas, and the rapidity with which a lime-wash, spread on a wall, will absorb this gas. One square decimetre of such a surface, in the course of six washings, has been known to absorb in four days as much carbonic acid gas as will produce three-quarters of a gram of calcium carbonate. This quantity was found by Liebig to be several times as great as that which is assimilated by the leaves and roots of plants growing on an equal area, in an equal space of time. It, therefore, seemed not unreasonable to answer the above question in the affirmative.

The evolution of carbon dioxide, which occurs at night, seemed to Liebig to be a natural but secondary incident. Since plants were supposed to absorb carbon dioxide during the day, but only to assimilate its carbon in sunlight, he thought it likely that at night some of the undecomposed gas would be returned to the air, together with water vapour; and he considered that the nocturnal absorption of oxygen, observed by Ingenhousz, was due to changes not at all connected with the life of the plant, caused by the action of the oxygen of the air on the organic substances composing its leaves, flowers, and fruit. In this Liebig was doubtless wrong. The evolution of of carbon dioxide and absorption of oxygen by plants at night are now recognised as the characteristic accompaniments of the processes of cell-division and cell-multiplication, by which their structure is increased, and which take place, at any rate, chiefly during night.

But on the main point he was right, for it is now fully recognised that the oxygen absorbed and the carbon dioxide set free at night, which misled so many botanists and vegetable physiologists, are far from being sufficient to counterbalance the carbon dioxide taken up, and the oxygen simultaneously set free during the hours of daylight, on which all plant life is founded.*

One of the reasons which had led physiologists to imagine that humus was particularly well suited for

* Davy showed the producing of oxygen from carbon dioxide by plants very elegantly, by growing grass under glass receivers standing in water, and containing air supplied with small additional quantities of carbonic acid gas from time to time. After the lapse of eight days, the air in the receiver was found to be very decidedly enriched with oxygen.

nourishing plants was the fact that it differs from wood chiefly in containing an excess of carbon. Like wood, it contains carbon, together with hydrogen and oxygen, in the proportions in which they combine to form water, but, relatively to the proportion of carbon, the proportion of water is less. Misled by this fact, the physiologists concluded that this similarity of composition between humus and the constituents of plants must favour the assimilating of the former by plants Humus had only to unite with water in order to form woody fibre.

In this, as Liebig pointed out, they went wrong, in consequence of trusting that most dangerous guide—an argument from analogy. They had no right to assume that the vital processes of plants are in this sense similar to those of animals. The vital process of a plant depends, as Liebig has taught us, on a change by which it recovers carbon from a simple inorganic substance. Plants, unlike animals, are not fitted to assimilate the products of plant life. On the contrary—as was, indeed, already known—when sugar, gum, or starch are absorbed by a plant, these compounds do not nourish it, for the very life of a plant consists in the elaborating of these substances. Humus, therefore, and the other products of life, can by no means be assumed to be fit for the support of plant life.

Here Liebig drew a clear and broad distinction between the vital functions of plants and animals, which was the corner-stone of his work in vegetable and animal chemistry.

But, although humus is not the food of plants, Liebig does not teach us to see in it mere dead, useless matter. When a leaf, a twig, or a tree falls, it enters on a new series of changes, in the course of which it

will ultimately pass once more into the condition of the simple oxidised compounds, which can afford nutriment for a new generation of plants. Humus represents one of the stages of this series of transformations. It is woody fibre in a state of decay. In contact with air, as in well-tilled ground, it undergoes steady if slow oxidation, and thus it becomes, according to Liebig, the source of carbonic acid gas for very young plants, the seedlings, to which it offers a supply of this necessary substance before the development of their roots and leaves enables them to take the latter from the air.

And, besides this, the carbonic acid gas, by entering into solution in the water which falls as rain, renders this more capable of disintegrating minerals, and thus promotes the processes by which the saline constituents of rocks become available for plants. Finally, the oxidation of the humus ultimately liberates the saline material of dead plants, so that they become once more fit to serve for the sustenance of their successors.

Humus, then, represents mainly certain stages in the primitive processes which connect one generation of vegetables with those which succeed it, so far as concerns those parts of vegetables which are not utilised by man.

These conceptions concerning the relations of plants and animals to one another, and to the air they live in, which we thus owe to Liebig, have now so completely taken their stand among the commonplaces of science, that most likely hardly any of the readers of this little book were unfamiliar with them when they opened its pages; and yet, probably, very many, if not most, were unaware how recently they

have become part of the equipment of mankind, by what steps they have been reached, or who it was that first traced out the few simple laws which underlie the relations of the two kingdoms of organic nature, by disentangling the intricacies of problems, which, simple as Liebig has made them seem to us, once appeared to be almost beyond solution.

Even in 1840, however, the most advanced chemists and biologists were by no means unprepared for such developments as those which we have just considered, for, as we have seen, the work of several then living investigators had already largely paved the way for them. Indeed, only about a year later Dumas, on behalf of himself and of Boussingault, delivered a lecture at the conclusion of his course of chemistry in the Medical School of Paris, entitled "*Essai de Statique Chimique des Êtres Organisés,*" in which they presented a *résumé* of their chemical and physiological researches, and propounded ideas closely corresponding with those previously expressed by Liebig, and in which an even broader view of the relations of the vegetable and animal worlds was brought forward and sustained. And before this, half a century earlier, Lavoisier, the father of modern chemistry, had anticipated the ideas which Liebig was, as we have seen, the first to establish beyond the possibility of further doubt.

"Plants," wrote Lavoisier in a document only discovered after the publication of the first three volumes of his collected works in 1862, "plants derive the materials necessary for their formation from the air which surrounds them, from the water, and in general from the mineral kingdom."

"Animals feed on plants, or other animals fed by

plants, so that the substances composing them are, in the last instance, always drawn from air and from the mineral kingdom."

"On the other hand, fermentation, putrefaction, and combustion continually restore to the air and the mineral kingdom the principles borrowed from them by plants and animals."

But whilst the discovery of this document must add to our admiration for the immortal Lavoisier, and sharpens, if that is possible, our regret for his untimely end, it rather adds to, than detracts from, the honour due to his illustrious successor that he should have originated independently, and in so nearly the same form, the brilliant speculations of his great fore-runner; and especially is this the case when we remember that, aided by the advance of knowledge, he succeeded in establishing the truth of these speculations and in building upon them one of the pillars of the natural sciences.

Plants do not consist of carbon only; they contain also, besides the elements of water, sulphur, nitrogen, phosphorus, and mineral matter. With regard to the sources of the hydrogen, the oxygen, and the sulphur of plants, Liebig considered that the first-named element, at any rate so far as that part of it which is in the non-nitrogenous compounds is concerned, is derived from water; that the oxygen is provided either by the water or by the carbonic acid gas; and that the sulphur has its origin in the sulphates which are present in sufficient quantities in soils.

Besides the chief elements mentioned above, there is another—viz. nitrogen—which exists in every part of every plant, and which cannot be supposed to be of less importance, although it is present in very

much smaller quantities. The question in what form does nature supply this nitrogen has for many years been the subject of almost constant research and discussion.

Nitrogen is, of all the elements, one of the least apt to enter into direct combination, and hence it has appeared improbable to chemists that the free nitrogen of the air could be the source from which plants gain their supply of it.

Liebig at first was of opinion that the source of the nitrogen of plants lay in the ammonia produced by the putrefaction of plants and animals, the remains of one generation producing by decay what was required for another. A good deal of nitrogen is thus returned to the soil in the form of ammonia in manure; some is yielded by the refuse of the crops, but on a given farm, unless all the products are consumed at home, there must be a loss in consequence of the selling of certain products. In any event some ammonia must also be lost during the process of decay (we have all of us recognised the odour of smelling salts in a badly-ventilated stable at one time or another); hence, practically, the nitrogen returned to the soil of a given farm by an agriculturist must always be less than was contained in the crops grown. Liebig therefore concluded that the ammonia brought down by the rain probably constitutes a very important source of supply. This opinion was soon challenged by Boussingault, who laid much more weight on the importance of manures as a source of nitrogen, and considered the supply obtained from the air to be of less importance.

In 1843, in a new edition of his book, Liebig may be said to have laid the foundation of the celebrated

"mineral theory" of agriculture. He also returned to the subject of nitrogen, and in his developed treatment of it he pointed out that whilst nitrogen can only be made to enter into combination with the other constituents of plants by the most powerful chemical means, and whilst this element is, moreover, frequently emitted by plants, ammonia, on the other hand, is remarkable, like water, for the number of the transformations which it undergoes, and for the various and opposite characters of the substances which it produces. As carbonic acid gas is the last product of decay so far as carbon is concerned, so is ammonia the form in which the nitrogen of animal bodies is returned to the air during putrefactive processes. The bodies of a thousand million men and of thousands of millions of animals are renewed every thirty years, and by their decay their nitrogen is converted into ammonia which escapes into the air, to be dissolved, for it is eminently soluble, by water in the form of rain, and so be carried back to the roots of plants, except in regard to that part of it which is washed more directly into the earth during decay, or is introduced fixed in the form of manure by the cultivator of the soil.

Although Liebig by no means denied that nitrogen may be supplied, with advantage, in the form of manure, he was led by the evidence then available to conclude that the ammonia in the air, like the carbonic acid gas, is by itself sufficient to supply nitrogen to many crops. This opinion was vehemently contested by his opponents; and partly because the data available were incorrect, and partly in consequence of misconceptions, it became the subject of a warm and prolonged discussion. Important as this discussion was to agriculturists, its details would not be interesting to

general readers; but it may be mentioned that this memorable dispute was only settled after Liebig's death by the important discovery, made not very long since by Professor Hellriegel and Dr. Wilfarth, that the power of the leguminous plants to assimilate free nitrogen* is dependent upon the presence of certain minute organisms which flourish in and around their roots, where they cause the production of tuber-like formations which swarm with the micro-organisms and abound in nitrogen.

But plants do not consist only of organic substances, and one of the numerous questions which puzzled the early workers in agricultural chemistry was the origin and function of the mineral parts of plants. The later upholders of the humus theory, though they could not absolutely deny the importance of mineral matters to plants, held that they were not exactly essential to their growth, but rather mere stimulants—useful perhaps—but still stimulants, and not a necessary part of their food. Some of them, indeed, held that the minerals were produced in the plants by their vital forces; and, astonishing as it may seem, in 1800 the Berlin Academy *awarded a prize* to an apothecary, one Schræder, for an essay in which he claimed that he had proved by actual experiment that plants produce mineral matter by their vital forces, and, although this paper was justly criticised by De Saussure, Schræder's views, as late as 1807, received a distinct measure of support from the English chemist, Dr. Thomson, who wrote that Saussure's remarks are "by no means sufficient to set aside the experiments of Schræder." Sir Humphry Davy, however, would by no means

* Established in recent years by Messrs. Lawes and Gilbert.

accept his results, and indeed detected the source of the error made by those inquirers who "adopted that sublime generalisation of the ancients that matter is the same in essence, and that different substances, considered as elements by chemists, are merely different arrangements of the same indestructible particles."

But Liebig first seems to have fully understood the importance of the mineral foods of plants, of which he gives us many illustrations, from which the following examples are taken.

Years ago it was the custom, in some parts of Germany, to permit the poor people to remove the leaves and twigs of trees from the forests, for use as litter for their cattle. It was found, however, that the trees suffered very much, far more, for example, than from the removal of an equal mass of wood alone. Why was this? The analysis of the ashes of twigs and leaves and of wood gives us the reason. The ash of the former contains much more alkali than that of the latter; evidently the removal of the twigs and leaves, by withdrawing álkali, robbed the trees of a part of their food—starved them, in fact. Had the leaves been allowed to remain, they would presently, by their decay, have restored their alkaline matter to the soil ready for further use.*

The tobacco plant and the vine give ashes which contain much lime. They do not grow well on soils devoid of lime; but when lime is added to such soils, the enriched soils become more fit for the growth of tobacco and grapes. We cannot avoid the inference that lime forms an essential part of the food of the tobacco plant and of the vine.

* It must not be supposed that this is the sole cause of the injury done.

Again, at Bingen-on-the-Rhine, the produce of the vines was at one time greatly increased by the use of nitrogenous manures, but, after a while, these were found to lose their effect, and the condition of the plants fell off so seriously that their possessor had great reason to regret the experiment. By the manure employed, as Liebig explained, the vines were hastened in their growth, so that in a few years they exhausted the soil of certain minerals, and these being absent from the manures employed, the plants were afterwards starved. Other vines on the Rhine, when treated with manures rich in potash and poor in nitrogen have lived for as much as a hundred years.

By such facts as these, Liebig made it clear that the mineral matter of the soil is as essential to the well-being of plants as carbonic acid gas, water and ammonia.

"Plants," he says, "live upon carbonic acid gas, ammonia, water, phosphoric acid, sulphuric acid, silicic acid, lime, magnesia, potash, and iron; many of them also require common salt. . . ."

"Manure, the excrements of the lower animals and man, does not act on plant life through the direct assimilation of its organic elements, but indirectly through the products of its decomposition and putrefaction. That is, by the transforming of its carbon into carbonic acid gas, and of its nitrogen into ammonia or nitric acid, Organic manure, which consists of portions or *débris* of plants and animals, may be replaced by the inorganic compounds into which it breaks up in the ground."

From what has been said it follows, as Liebig pointed out, that we must replenish the soil by adding

whatever minerals have been withdrawn from it by our crops if we desire to avoid exhausting it.

Since the supplies of carbon dioxide, of water, and of ammonia are, for the most part, beyond our control, and since, moreover, these are ample, it appeared to him evident that the chief work of the agriculturist, after keeping his soil in proper condition by tilling it, is to prevent loss of mineral matter, or when that is inevitable, to restore the fertility of the soil by adding what is required.

There has been, as we have seen, much discussion as to whether Liebig was right in supposing that the air does really yield to the plants a sufficient supply of nitrogen; but this is, after all, a detail, and we may say now, fifty years after the first publication of Liebig's pioneering work, that nothing has been added to, and nothing has been taken from, the fundamental principles to which he then drew attention, and on which he based his theory of a rational husbandry. To-day Liebig's teachings are, in their main features, universally received. But, in spite of his reputation, they were not generally accepted by agriculturists when he first announced them. Nor is this surprising. The education of most agriculturists was then at a low point. Science, or what passed for science, had done very little for agriculture in the past. The difficulties in the way of applying Liebig's ideas were great, and at first, owing to imperfect data, serious mistakes were made which led to many failures. Hence it was only to be expected that practical men would, for a time, consider them to be mere newfangled notions; that they should regard them with considerable suspicion, and cling to the ideas and practices, which were many of them wise, handed

down by their forefathers. The publication of "The Chemistry of Agriculture and Physiology" only heralded, at first, a prolonged period of renewed experimenting and warm discussion. Liebig did not, however, allow this to discourage him, but, for many years afterwards, he devoted a large part of his life to studying the practical application of his theory, to gaining the serious attention of practical agriculturists, and, above all, to bringing about such a just appreciation of his views as should ensure their ultimately being intelligently applied in practice.

In the first place, countless analyses of the ashes of plants were made at Giessen by Liebig and his assistants. These showed the presence of different minerals in every species, that each species requires from the ground the same class of salts, and hence that it must sooner or later exhaust the supply of these salts in a given plot, and render it unfit for the growth of the species in question unless fresh supplies are provided.

Liebig attempted to give the necessary supplies in the form of "Mineral Manures," and soon set to work to study practically the effect of mineral manures on a large scale. In the year 1845, previous experiments in a garden having proved unsatisfactory, he purchased from the town of Giessen about ten acres of barren land—a sand pit, as he says, which surpassed all the land in the neighbourhood in its barrenness for ordinary cultivated crops; in the year this land hardly grew so much fodder as would have sufficed for a single sheep. It consisted partly of sand, partly of coarse quartz and pebbles, with strata of sand and some loam.

Some of the soil was first tested by sowing it with

seeds in pots after enrichment with some single mineral manure, with the result that not one of the plants raised got beyond flowering; this showed that the soil was bad enough for his purpose of testing the value of minerals as manure.

A number of mineral manures were then prepared for him according to prescriptions based on his analyses, and these were spread over the land; next he sowed on different subdivisions of it wheat, rye, barley, clover, potatoes, turnips, maize. In some cases he added sawdust to the manure, and in one case he used stable manure; otherwise, no ammoniacal manure and no mineral matter was employed, except that to one plot he applied some forest soil and to another a mixture of forest soil and mineral manure. Even in the first year he had a harvest; the best results were given by those plots in which mineral manures were mixed with forest soil or stableyard manure. This, as he says, enabled him to correct his earlier ideas of the functions of humus, which by its decay renders an extra supply of carbonic acid gas to the plants that is especially valuable at the early stages. Gradually, without any further supply of manure except mineral manure, the land so improved in productiveness that in the fourth year his crops excited the wonder of all who had known the original state of it.

In 1849 this little farm was purchased by his gardener, who was then able to farm it with profit, raising some cattle on it yearly and getting such satisfactory crops of corn that in 1853 a neighbouring farmer wrote: "With us the wheat crops are very poor, but on the height (Liebig's plot) they have harvested from three fuder of rye twelve simmer, while I, from three fuder of the best rye, have only

got five simmer. If you were to see it, you would be astonished; it is truly wonderful."

Of course, in order to maintain the fertility of this plot it was necessary that the capital of minerals introduced during the first four years should be carefully husbanded, and year by year returned to the soil in the form of the manure produced on the farm, and that little or none of it should be removed from the farm by selling the crops. Under these conditions those ten acres of land became, as it were, condensers of carbon and nitrogen from the air. It was this experiment, as well as the estimates of ammonia in rain water, which led Liebig to form the opinion that it was possible, by giving the soil proper physical quality and composition, to bring about a state of things in which sufficient ammonia to maintain its fertility can be collected or condensed from the air.

The experiment was, of course, a costly one. It rarely happens that an experiment is directly remunerative, but it served its purpose of testing the agricultural doctrines of Liebig, and its success enabled him, amid frequent difficulties, to work patiently in other directions.

One great difficulty was, as Vögel says in his memorial address, "the want of results" from the mineral manures when they were applied in the ordinary course of farming. The attempt to compensate the soil for the withdrawal of crops by the addition of suitable mineral matter seemed to fail, because the form in which the salts were at first supplied was the wrong one. It was supposed that if soluble salts or solutions of them were applied to the land, they would be ineffective because they would soon be washed out of reach of the roots of the

plants by the rain, and therefore no pains was spared in attempting to render the mineral manures insoluble, or, rather, so far insoluble, that the moisture in the soil could only gradually absorb them for the use of the plants. For this purpose suitable mixtures were first melted in huge pots, and then ground to a powder by means of mills, before they were put upon the land.

This plan was, however, a brilliant mistake which a practical farmer might have avoided. For when these mixtures were applied to the soil, they were found to be almost without effect, or, at best, their usefulness only showed itself after long years of delay. The single ingredients when applied alone acted, but the same substances mixed would not act. There was evidently something wrong.

Liebig, convinced that the error must be in his practice, that his hypothesis was a sound one, fortunately did not allow this failure to put an end to his experiments, with the result that the failure led him in the end to recognise the agricultural importance of a certain quality of the earth which enables it to protect, as it were, the plants from being robbed of their mineral food by the rain as it percolates through the soil. While Liebig was so busy trying to make his manures insoluble, nature was ready to do it for him in the most suitable manner.

It appears that earth is capable of withdrawing, to some extent, soluble salts from their solutions, but the importance of this fact—which had been known for some time—in relation to the nourishment of plants was overlooked until about 1850, when Liebig's attention was directed to it through some novel experiments made in England by John Thomas Way on the absorptive power of soils. Liebig at once recognised

their importance. Those who have ever attended a course of lectures on chemistry will remember that many coloured solutions, when filtered through a layer of finely divided animal charcoal, lose their colour, in consequence of the power possessed by the finely divided carbon of withdrawing the coloured substances from the solutions. It is possible, at any rate, in some cases, to recover the substances thus absorbed by charcoal. Now, arable soil is in this respect very much like charcoal. The dark drainings from a manure heap, when filtered through a layer of good soil, flow away without colour and without smell; the organic matter, the ammonia, and the salts which it held in solution are all more or less completely withdrawn from it by the soil. The various substances do not, however, appear to be destroyed, they are held by the soil ready for the use of plants. The discoveries made by Way led Liebig to himself perform a series of experiments, in which he investigated this property of "ground absorption," as it has been called, by which the saline matters in the soil are at once protected from the action of the rain and held ready for absorption by the roots of plants.

Liebig's results, besides confirming the statements of the English observer, enabled him to declare at last what is the proper mode of applying the mineral manures; they also helped him to understand the root action of plants, and to explain clearly why it is that the whole available quantity of a manure is not taken up by the roots of the plants in a single season. Why it is, for example, that if ten grains of a phosphate are taken up by a crop, many times that quantity must be present in the soil. It is because the roots of a single plant do not touch all the soil about it, and

because they only exhaust those parts with which they actually come into contact. Liebig's excitement when he recognised the effects of ground absorption was very great. In his writings at this time he frankly confessed the mistake into which he had fallen. This new discovery, he declared, was like a new life to him, explaining as it did all the processes of agriculture, and why, whilst each single salt had succeeded as a manure, combined they had failed.

Regret has sometimes been expressed that Liebig devoted so much valuable time and effort to the practical application of his mineral manures. It has been questioned whether he would not have done better if he had left to the practical farmers the duty of applying the principles he himself had traced out for their guidance. Had he done so, there can be no doubt but that he would have saved himself much annoyance and anxiety; he might even have hastened the success of his ideas, since it is possible that a practical farmer might have avoided the mistake which led Liebig to spend so much time in attempting to render his soluble salts insoluble.

But, on the other hand, if Liebig had stood aloof from the testing of his opinions, science and agriculture would alike have been the poorer—science, because in that case the full recognition of the importance of ground absorption would almost certainly have been delayed; agriculture, because in his practical work Liebig gave the agricultural world a splendid example of what experimental agriculture ought to be, at a time when such an example was sorely needed. Indeed, when we remember that throughout his whole career Liebig was as greatly distinguished for his enthusiastic services to educa-

tion as for his work in pure and applied science, and when we consider his keen sense of the urgent need that farmers should play at follow my leader no longer, but learn how to make their own experiments on their own farms, I do not think we shall venture too far if we assume that it was largely with the object of showing them how this should be done that he entered upon his labours in experimental agriculture. However this may be, no one surely will deny that the world is greatly the richer for the outcome of this part of his many-sided activity.

Liebig showed on many occasions his desire that farmers should think more for themselves. In the preface to his "Natural Laws of Husbandry," published in 1863, he called attention to the fact that no progress could be made so long as agriculturists continued to allow themselves to be guided merely by the facts observed in their own neighbourhood, or at most by the system of some recognised authority, and he elsewhere deplored the frequent existence of such a state of mind as that which had led a landowner to write to the eminent agriculturist, Thaer: "If I receive a letter from you this evening to fire my buildings, before night they shall be in flames."

In the same book, writing on the subject of farmyard manure, Liebig returned to this subject, and after showing that the rotation of crops which suits one field may not suit another equally well, he said: "If farmers would only make up their minds to acquire by experiments on a small scale an accurate knowledge of the productive power of their land for certain kinds or classes of plants, a few more experiments would readily enable them to discover what nutritive

substances the land contains in minimum proportion, and what manuring agents ought to be applied to ensure the production of a maximum crop."

"In matters of this kind," said he, "the farmer must pursue his own course . . . he must not put the least faith in the assertion of any foolish chemist who wants to prove to him analytically that his field contains an inexhaustible store of this or that nutritive substance."

One cannot read this book without perceiving that Liebig's great desire was not merely to inculcate correct rules of husbandry, nor to attempt to induce the agriculturists to lean more on the chemists, but to lead them to take an intelligent interest in their own work, and to study the bearing of science on its ever-varying details.

He found most agriculturists badly educated, and either governed by tradition or the slaves of particular leaders. They had lost the power of distinguishing opinions from facts. They had no idea how to make an experiment. He hoped to help to put an end to this by inducing the rising generation to take an active part in the attempt to apply scientific methods to their art.

Even those who most vehemently dissented from some of Liebig's conclusions on special points have always admitted in the warmest terms the magnitude of the service he rendered to agriculture, by the masterly review of the then existing knowledge, and by the sagacious generalisations which he brought forward in his earlier works on agricultural chemistry. And when he published his mature views in 1863, in "The Natural Laws of Husbandry," his fundamental doctrines were already, in regard to most of

their main features, generally accepted. It was then no longer necessary that he should demonstrate the errors of the humus theory, and, on the other hand, the true value of humus was better understood. In 1863 there was no need that he should insist upon the necessity for mineral matter in the food of plants, nor that he should direct attention to that interdependence of plants and animals, which alone has enabled countless generations of each class to play their parts in the past history of our globe, and which alone makes the continued existence of either possible. By this time, also, the power of arable soil to withdraw from solutions the food materials most essential to plants was fully recognised, and correct views of the action of roots accordingly had become possible. In the new book, therefore, Liebig was able, after many years of experimenting and reflecting, to devote himself more especially to the task of explaining how the new knowledge and corrected ideas should be applied to the practical objects of agriculture. He could now attempt, in fact, to bridge the gulf which had so long separated science from practice.

First among the objects of the agriculturist must be placed the maintaining of the permanent productiveness of the soil. To this subject Liebig especially directed attention at this stage.

The following remarks will serve to introduce and make clear the last part of Liebig's teachings in agricultural chemistry to which it is possible to refer—viz. to the tendency of the system of farming adopted by Western peoples to exhaust the soil, and so ultimately to bring about their own extinction or dispersal more or less completely.

It must be understood that a fruitful soil consists (1) of the arable surface soil, and (2) of the subsoil. The former contains especially that part of the nutriment of plants which is held there in consequence of the absorbing power of humus *—that part, in fact, which is immediately available for nourishing the plants. The fertility of a soil depends on its abundance and on its suitability for the crops that are to be grown.

The soil also affords another source of food, but in this case the food is not immediately available. It is stored up in the form of compounds which are not soluble in water, and is only gradually brought into the available condition by the process which we call "weathering." This process takes place slowly. It is accelerated by the mechanical operations of agriculture, in the course of which the insoluble substances are gradually disintegrated, so that the soluble parts slowly pass into solution under the combined action of air, water, and carbonic acid gas. As this saline food is liberated, if the conditions are favourable, it passes into what Liebig called a state of "Physical † combination" with arable soil. It then becomes an immediate source of supply for the use of plants.

Each soil has its own degree of power for absorbing food, and this constitutes one of the differences between soils from the agricultural point of view. When manure is applied to the surface of many soils, most of the food it conveys is absorbed by the first few inches of the soil, and very little gets to the subsoil.

* Other constituents also contribute to ground absorption, notably oxide of iron and certain silicates.

† Physical, because he believed that the salts retain each their individual properties.

Consequently, it is difficult to replenish an exhausted subsoil by manure. It is advantageous to distribute the manure by ploughing or digging, and the other operations of the husbandman, because, as each particle of the soil is limited in its absorbing power, it is only by mixing the various parts that the whole mass can be brought to the saturated state in which it best serves to nourish plants. Evidently, also, the working of the soil will tend to an equal distribution of the food which is gradually set free from the insoluble components of the soil by weathering.

Food in the so-called state of "physical combination" is at once available for the use of plants, because it is not held sufficiently strongly by the soil to resist the absorbing power of their roots. But it is retained firmly enough to prevent its rapid removal by the rain water which percolates through the soil.

The above facts enable us to understand why rough uncultivated land will often sustain perennial plants when it fails to afford satisfactory crops of the summer plants grown by the farmer. The perennials absorb their food at a slower rate than the others, but they continue to absorb it for a longer period, hence, they can take advantage to a certain extent of the slow production of food by weathering; they do not altogether depend upon the amount of food in the more readily available condition. But summer crops must absorb quickly. They require far more food in their short lives than the perennials need in an equal time; therefore, these only flourish on land which, either in consequence of prolonged cultivation, or as the result of natural processes, offers them an ample supply of immediately available food such as is only

to be found in land that is rich in "physically combined" nutriment.

This, again, explains to us the great usefulness of weeds in waste places. Weeds slowly accumulate food from barren soils; and if they are not removed, by their decay after death they return this food to the soil in such a form that it enters into the so-called "physically combined state." In this way successive generations of weeds gradually enrich the soil, and prepare it for bearing more valuable crops.

The fact that the constituents of soils exist in the two above-mentioned forms makes it very difficult to ascertain the value of land by means of chemical analysis.

By means of analysis the chemist can find out for the farmer how much food of each kind his soil contains—how much potash, how much phosphoric acid, and so on; but when it comes to ascertaining what proportion of the various components is in the chemically combined state, and how much in the "physically combined," or available state, analysis, in Liebig's time, broke down, and I fear it must be added that it breaks down still. In the early days, before the distinction between the available and the unavailable food in the soil had been made clear to us, this circumstance caused great confusion, and opposed a serious obstacle to progress, because the productiveness of soils was often found to be apparently quite unconnected with their composition, which naturally weakened the confidence of the farmers in the usefulness of science to their art.

We must now pass to the question of the exhaustion of the soil by the European methods of cultivation, to which Liebig gave much consideration.

The principal problem for agriculture is how to replace those substances which are taken from the soil by crops, and which are not found in the atmosphere. If the manure applied does not replace what is taken by the crops, the fertility of a field, of a farm or of a country will decrease; if, on the contrary, more is given to the field than is taken away, its fertility will increase. In a primitive state, a nation of farmers who till their ground well and collect and return to it every trace of manurial matter produced by those who consume its products, need not fear they will exhaust their ground. The Japanese afford us an example which is to the point. In Japan a teeming population has been supported for centuries by means of a simple agriculture, supplemented by fishing. This consists in returning to the soil year by year all that is taken from it for food, except only those parts which pass into the air and so find their way back to the earth by natural processes.

The only rational agriculture, according to Liebig, would be a system in which, at least, everything that is taken from the land should go back again. If we can enrich our soils from without, and so add more than we take, so much the better, provided that we do really enrich them by adding all the necessary elements of plant food in their proper proportions, and do not merely stimulate the soil by adding one or two elements only, for this cannot fail to ensure a premature exhaustion of those components of the soil which we do not add.

This ideal of Liebig's is perhaps reached in Japan and by the Chinese, but not in Europe. In all Western civilised countries a very great part, and sometimes the greater part, of the food produced on

the land is carried away to be consumed in the towns or in distant countries. Thus the greater part of the mineral matter removed by each crop is never returned to the land. The carbon, the hydrogen, and some of the nitrogen are indeed ultimately returned through the atmosphere, but, by our systems of sewerage, the mineral matter and a part of the nitrogen are often cast away and lost to us. Even the carbon does not go back to the land to pass through the humus stage, and so does not again play its whole part in the processes which produce fresh supplies of food. In China and Japan this terrible waste is avoided, but not in the West.

An ingenious system for staving off the evil day has made it possible for European farmers to continue to raise crops for a long while. By adopting a certain rotation of crops, by feeding animals on the land so as to return a good deal in the form of manure, and by importing food-stuffs for their animals from foreign countries, farmers have been able to raise crops of corn, and also meat, and to sell them off their farms without perceiving very obvious signs that the land is being permanently exhausted. But what is it that really happens?

If we grow a crop of corn and remove it from the farm, the surface soil will lose a certain portion of the constituents that go to the formation of corn. These must be returned in the form of manure; and if this be done, the subsequent crops may reach the level of those first obtained. To provide this manure, it is the practice to grow such crops as turnips, clover, and grass, and to feed cattle or sheep on these on the farm. A part of what these cattle consume in their food remains in their bodies, and this

part, sooner or later, is removed from the land, and, except perhaps so far as their bones are concerned, like the corn crop, is mostly lost to it. Another and very large portion of the nutritive substances drawn from the land by these creatures is returned to it in the form of manure, and this enables the arable surface soil to again support corn crops.

But it is evident that the food constituents thus supplied in the form of manure to the surface soil were not created out of nothing by the fodder plants, nor in the bodies of the animals. There is not the slightest reason for supposing that this is possible. The truth doubtless is, as Liebig insisted, that the deep penetrating roots of the fodder plants enable them to extract nourishment from the subsoil, which is then partly carried away and consumed in the form of milk, cheese, meat, and partly restored to the surface soil in the manure provided by the cattle, to be again presently withdrawn in the form of corn. Only a small part of the nutritive substances returned in this manner can ever reach the subsoil again. Most of them will be absorbed by the upper layers, whence they will soon be carried off in the subsequent corn crops.

Under such a system as this, said Liebig, the corn crops may be kept up, they may and even do increase, and this increase may go on for a long while ; but, unless we assume that the stock of nutritive matter in the subsoil is infinite, there must be a limit. Sooner or later the subsoil of each field, which is thus drained of its phosphoric acid, potash, lime, etc., must begin to lose its productive power for the fodder crops, and then the nutritive substances taken away from the arable soil in the form of corn can no longer be

returned to it from the stores which at first existed in the subsoil. There are various devices for delaying this dreadful consummation, but sooner or later, according to the quality of the subsoil, exhaustion must ensue, and then the corn crops will decline, and continue to decline, unless all the mineral matter, etc., which is taken from the soil be once more returned to it.

Of course, the progress of the decline is slow, for the store to be drawn upon at first is considerable, and the results are only felt after many generations. They may be deferred by gathering fallen leaves from the woods, by breaking up new ground, by drawing on outside sources of manure, such as guano, etc., but all these methods are only palliatives. In face of the fact that corn can only be grown if we replace at short intervals what each crop has removed from the surface soil, it is a crime against human society, urged Liebig, to assume that the fodder plants and the subsoil are not subject to the same law, and that the former will constantly find in the soil all the conditions of their growth.

To continue to draw on the store of mineral food in the soil of a farm without replacing it is like drawing out money from the bank for daily expenses and never troubling to earn any more to replace it before it is all gone. Or rather, perhaps, as we are wasting a store which, properly used, would serve for the support of untold future generations, the system which enables the farmer to sell successive crops off his land without returning their equivalent may be compared to a skilfully-contrived robbery, by which the fathers rob their own children.

At present, owing to the opening up of virgin

soils in various parts of the world, this question has seemed to become less urgent, for the moment, than it appeared to Liebig, so far as the general population is concerned. This, however, is but a temporary state of affairs; at best it only gives us a breathing-time. The question must be faced sooner or later. For the agriculturist the problem how to bring back to the land the nutritive matter taken from it to the towns is not one whit less important now than when Liebig wrote. At present we are gradually wasting a capital which we ought to make increasingly valuable, and which no human power can restore when once it is dissipated. The problem is not insoluble. It has been solved by races we are pleased to regard as almost barbarians. Till we, too, attain a solution suited to our conditions, we remain mere robbers and wastrels.

The outcome of Liebig's work in agriculture must by no means be measured solely by the new facts and new views of facts contained in his writings. He was not only the founder of a new school of agricultural chemists, but so large a proportion of the last generation of agricultural chemists came directly or indirectly from his school after he laid down the foundations of his doctrine that, without disparaging the valuable work done by others—for example, by Boussingault in France and by Lawes and Gilbert in England—he may almost be said to be the founder of agricultural chemistry itself as we know it to-day. And it cannot be doubted that we owe the existing machinery for agricultural research and teaching very largely indeed to the widespread interest he awakened in scientific husbandry.

In his well-known address to the Chemical Section

of the British Association, in 1880, Dr. Gilbert called attention to this fact, when he pointed out that it was only after the publication of Liebig's first reports to the Association that the Royal Agricultural Society first appointed a consulting chemist, Dr. Lyon Playfair (now Lord Playfair), Liebig's pupil, being the first holder of the appointment. Abroad, both in Germany and elsewhere, numerous "agricultural experimental stations" have been established. These owe their origin to the teachings and influence of Liebig. The first of them was established at Möchern, near Leipzig, in 1851–52. Twenty-five years afterwards there were seventy-four such stations in Germany, sixteen in Austria, ten in Italy, and altogether on the Continent one hundred and twenty-two. At each of these was a chemist, often with one or more assistants. The officials of these institutions are charged with the duty of examining and reporting on such substances as the manures, food-stuffs, and seeds that come into the market. Agricultural research has also been a characteristic part of their work. Their activity in this direction has covered a wide field. Whilst some have occupied themselves with the study of soils, manures, vegetable physiology, animal physiology, feeding experiments, vine-culture, wine-making, forestry, and milk-production, others have, according to their localities, specially investigated such subjects as fruit-culture, olive-culture, the utilising of bog and peat land, or the producing of silk, spirits, etc.

Besides this work in Europe, a good deal is now being done in the United States by the workers in numerous agricultural stations.

In Great Britain much less has been done than abroad. Neither the State, nor the great landowners

as a class, have taken the lead in the matter—with the result that a few years ago the German Empire already possessed above one hundred high schools, middle schools, and lower schools, with a full provision of experimental stations attached to them, besides more than a thousand others where the principles of agriculture were taught to all classes; whilst there were in England at the same time only two agricultural colleges, one each in Scotland and Ireland, with a laboratory of agricultural chemistry in London for higher students, and South Kensington courses for those of a lower grade. Since that time there is reason to hope that a happier state of things has been started by the County Councils. But there is still much lee-way to make up, and not only are our farmers terribly below Liebig's ideal of a race of husbandmen acquainted with the principles of their art and capable of intelligently applying them, but their opportunities of getting the scientific training and knowledge, which alone can cure the present evil state of things, are still terribly insufficient.

CHAPTER VII.

PHYSIOLOGICAL CHEMISTRY.

Origin of Animal Heat—Classification of the Components of Food—Plants elaborate the Nitrogenous Components for Tissues of Animals—Importance of Albumin in Food of Animals—Use of the Non-nitrogenous Components of Food—Liebig's Classification of Food-Stuffs into the "Plastic Foods" and "Respiratory Foods"—"Plants accumulate Force"—Origin of Animal Fat—Bischoff on Liebig's Contributions to Physiology—Relation of Nitrogenous and Non-nitrogenous Food to Work—Method of Studying Production of Urea in the Organism—Motions of the Juices in the Animal Body—Researches on Flesh, Creatine, Creatinine, Sarcosine, etc.—Mineral Matter in Flesh and Blood—Chemistry of the Cooking of Flesh—Extract of Meat—Vital Force—Objects of Liebig in his Physiological Work.

LIEBIG was very conscious of the vast importance of physiological studies, and at one time he even contemplated occupying himself with medicine; and though this idea was never carried out, a great part of his life was devoted to efforts to advance medical science through physiology. His first work on animal chemistry constituted the second part of his Report to the Chemical Section of the British Association, in 1842. In it he traced the applications of organic chemistry to animal physiology and pathology.

Just as Lavoisier during the latter half of the previous century had laid the foundation of what may almost be called a new science, by the success with which he applied to chemistry methods that

had long before been employed in physics—that is, the use of weights and measures *—so Liebig aimed at applying the new and altered views that had been introduced into organic chemistry to the elucidation of the problems of physiology and pathology. Ideas derived from chemistry had previously often been successfully applied to the problems presented by the medical art. But for some time before Liebig directed his attention to physiology, physiologists had concerned themselves especially—perhaps almost exclusively—with the study of the forms of organised bodies and of the phenomena of motion within them, whilst the study of the uses and functions of the different organs and of their mutual connection in the animal body had, to a great extent, fallen into the background. Their researches had given most valuable results, but they yielded, as it seemed to Liebig, no conclusions calculated to give a real insight into the vital processes. And it appeared to him that the greatest hope of advance in this direction was to be found in once more making chemistry the handmaid of physiology.

It was natural that Liebig, the great master of organic chemistry, should apply himself to the task of bringing the new organic chemistry into the service of the sister science.

A hundred and fifty years ago, chemistry and physics had comparatively little in common. A hundred years afterwards it had become difficult to draw a line between them; Liebig looked forward to the time when chemistry and physiology

* Lavoisier did *not introduce* the use of the balance into chemistry, as has often been stated. No one, however, advanced chemistry by the use of this instrument so much as he did.

would similarly be so fused together, as it were, as to make it difficult to clearly separate the one from the other. "In the hands of the physiologist," he says, "organic chemistry must become an intellectual instrument by means of which he will be enabled to trace the causes of phenomena invisible to the bodily sight." It was the use of this new instrument that he wished to illustrate by his "Organic Chemistry in its Applications to Physiology and Pathology." The production of this book provoked at least as much interest, and even more opposition, than his writings on agriculture. This was probably due, not only to what he said, but often very largely to how he said it. For it must be confessed that Liebig was severe upon what he regarded as the neglect by physiologists of the quantitative methods which had for some time been in use in chemistry.

Even before Liebig's great contributions to physiological chemistry were published, there was evidence that the reign of the empirical method in medicine was coming to an end. The rapid progress of chemistry, and especially of organic chemistry, could not fail to attract the attention of physiologists, and it conveyed to them the lesson that the true path of advance is through the combined employment of experiment and observation.

When it is observed that a lean goose weighing four pounds gains in a few weeks three and a half pounds of fat from the consumption of twenty-four pounds of maize, the observation, taken by itself, might well lead us to conclude that the twenty-four pounds of maize contained in them three and a half pounds of fatty matter. At the best it leaves the question completely open whether a goose can produce any fat

from the non-fatty substances in its food. But if we appeal to an experiment, if we feed the goose on food of known composition—that is to say, if we analyse samples of the food, and find that the amount of fat in the food eaten is considerably less than that which is accumulated by the goose, we shall definitely settle the question. Similarly, by varying the composition of the food administered to our goose, we might hope to decide which of its components was the source of the fat produced. By similar experiments on other animals it would become possible to decide the question whether or not fat can be produced in the animal body from carbohydrates. Here we have a simple illustration of the experimental method as it may be applied to physiology.

During the decade which preceded the appearance of Liebig's book, there had been, as said above, distinct indications that some physiologists perceived a new road opening before them. Johannes Müller and Tiedemann, for example, had already expressed themselves in support of the experimental method, and in 1841 Bischoff had perceived, with prophetic vision, that the direction which organic chemistry was then taking was of the greatest importance to physiology, and in this connection he had already recognised the value of Liebig's work. Moreover, various useful steps had been taken. In spite of the almost universal reluctance of investigators before Liebig's time to abandon the idea that in biological processes all manifestations of chemical and physical action are in some mysterious way modified and overruled by a special vital force, Wöhler, as we have seen, had weakened their hold on this idea by producing, from mineral sources, urea, a most

characteristic product of animal life. Prout had observed the presence of hydrochloric acid in the stomach, Gmelin and Tiedemann had investigated the processes of digestion, Wöhler had observed that salts of organic acids in passing through the animal body are converted into carbonates—that is, into the very substances which they would form if consumed in combustion—and many other isolated but valuable contributions to animal chemistry had been made.

Thus, when Liebig turned his mind to the study of the chemistry of physiology, he found a mass of facts ready to his hand, and, above all, a soil which was not altogether unprepared to receive his ideas. Whilst, in addition, his great authority as an organic chemist made him secure of an audience which extended even far beyond the ranks of those engaged in the study of science and its applications. It was, therefore, under auspicious conditions in every respect that he attempted the task of elucidating the action of chemical and physical laws in the life-processes of animals.

The most generally interesting and most wide-reaching of all Liebig's teachings in physiological chemistry are doubtless those which deal with the relations of plants and animals to each other and to their environment. These relationships have, however, already been partly discussed in the chapter on agricultural chemistry; and therefore, before returning to them, it will be best to illustrate his physiological work by examples of his treatment of some of the more important questions which go to make up the subject. I select for this purpose not simply those questions on which his conclusions are still accepted as true, nor those on which his opinions have become

truisms, but rather those which will, on the whole, convey the clearest idea of the scope and method of his work to readers who are not already acquainted with this branch of science.

One of the most interesting, and one of the most important, questions which Liebig examined was that of the origin of the heat of the animal body.

The temperature of every human being in a good state of health may be said to be practically constant. It only varies during the twenty-four hours from 36·5° to 37·5° C. (98° to 99° F.); and so it is with many other animals, including most mammals and birds.* The temperature varies a little in different parts of the body; it is a little higher internally than externally, for example. This uniformity of temperature is maintained in spite of the fact that a constant loss of heat occurs by radiation, evaporation, and the expulsion of warm air from the lungs in expiration, etc. From the data given in Professor Halliburton's "Chemical Physiology" I have calculated that a human being in a state of rest loses enough heat in twenty-four hours to melt, approximately, sixty-six pounds' weight of ice. What is the source of all this heat, which from one person, in the course of a long life, would suffice to melt a small iceberg? By what processes is it generated?

Long before Liebig's time Lavoisier perceived the analogy between the processes of combustion and respiration; for both of them air is required, and by both of them carbonic acid gas is formed. Lavoisier had suggested in a paper, published jointly with Laplace, that the heat evolved by the animal

* Even the cold-blooded animals have a temperature slightly above that of their environment.

organism corresponds to the heat of combustion of the carbon and hydrogen which are taken into the body in the form of food, and they had made some experiments on the subject. Subsequently this heat was measured with the greatest degree of exactness then attainable. The results showed that whilst the explanation of Lavoisier and Laplace would account for the greater part of the heat given out by animals, it would not account for the whole of it; about 10 or 11 per cent. remained, which, it seemed, could not have come from the combustion of the food.

The mental attitudes of the older school of physiologists, and of Liebig and his successors, are well illustrated by their respective methods of treating the problem which was thus introduced, and which urgently demanded an answer.

The older school accepted the result, and at once proceeded to invent a theory to account for it. They suggested new sources of heat, such as nervous actions, or friction within the animal organism.

Liebig, on the other hand—though then unaware, it is said, of the principle of the conservation of energy—perceived the emptiness of these theories, which only raised new difficulties. He, therefore, carefully reconsidered the whole subject; and when he found, after correcting the calculations in certain respects, that a surplus there surely was, proceeded at once to search for a possible source of error in the experiments themselves. He found such a source of error in the fact that the calorimeter* employed was

* A calorimeter is an instrument for measuring "quantities of heat." A unit of heat may be taken to be that quantity which will raise one gram of water from 0° to 1° C.

one in which the animals experimented on might very conceivably have been cooled below their initial temperature, so that the heat taken up and recorded by the instrument might very possibly not have been derived solely from the respiration of the animal; part of it might have been due to the cooling of its body. In summing up his views on this subject, he declared himself to be finally convinced that the whole of the sensible heat produced in an animal body could be accounted for by the processes of oxidation which occur within the organism.

To-day the correctness of Liebig's decision is so completely established that it may be regarded as one of the truisms of science. Whilst we now certainly admit that friction, nervous activity, etc., may actually give rise to heat, we recognise that these actions themselves are only intermediate steps, as it were, between the chemical changes concerned and the heat ultimately measured, and that, therefore, heat from such sources could not lead to the production of more heat by the life process of the animal than that which corresponds to the chemical changes which occur within it.

But Liebig's conclusion was by no means accepted at once by his contemporaries. At first they rose up against him, and said it was not true. The physicians thought they had discovered special sources of heat in the animal body. Even the chemists did not support him. Berzelius wrote, near the end of a well-known letter on the subject: "You will easily see, my dear Liebig, that you are here standing on hollow ground, and that whatever you build must, in spite of your talent, sooner or later fall to pieces." Afterwards, when it had become plain

that Liebig was right, some of his opponents declared that, though his conclusion was true, it was not new, but only what Lavoisier and Laplace had said before him. This was true enough in a sense, but it was unfair. For, as Bischoff has pointed out, Liebig by no means merely developed the idea of his predecessors. His merit is that he found false doctrines generally accepted, and overthrew them almost single-handed in the face of the strongest opposition.

In some of his most important contributions to physiology, Liebig discussed the classification of foods, and dealt generally with the chemistry of the food of animals.

The components of plants on which animals feed include, besides a small amount of mineral matter, two great classes of organic compounds—first, the non-nitrogenous substances, such as the carbohydrates, sugar, starch, and cellulose, together with fats; secondly, the nitrogenous compounds, of which the most important are the *albuminoids.* The gluten of flour, vegetable albumin, and vegetable casein, which occurs especially in the seeds of peas, beans, and similar plants, belong to the nitrogenous group of substances.

Liebig's attention having been drawn, through the writings of Mulder, to the similarity between the nitrogenous constituents of plants and animals, and to the probability that animals depend upon plants for these substances, and are themselves unable to form them, he and his pupils re-examined these substances. They found that the nitrogenous compounds which form the main part of the nitrogenous food of the graminivorous animals are indeed composed of the same chemical elements—viz. carbon, hydrogen, oxygen, nitrogen,

with a little sulphur and phosphorus, united in very nearly the same proportions by weight as those which are present in animal fibrin, and in the other albuminous constituents of the blood. Their properties were also examined, and, so far as could be then ascertained, the two classes of substances were found to correspond closely in their chief qualities. They were not different substances with the same composition, but actually identical, or nearly identical, compounds.

Now, the graminivorous animals, as we all know, form a link between the plants which they eat and the flesh-eating animals—in which class, for convenience, we may include man—which eat them; consequently this discovery threw floods of light on the mutual relations of the plant and animal kingdoms of nature.

Since vegetable albumin and fibrin are so little different from animal albumin and animal fibrin, it seems to follow that the vegetables produce in their organism, as it were, the blood of the animals.

It is well known that nitrogenous food-stuffs are absolutely essential for the well-being of animals; the flesh-eating animals in consuming the flesh of herb-eaters consume, strictly speaking, only the vegetable principles which have first served for nourishing the latter. Vegetable albumin and fibrin, in short, according to Liebig, play a similar part in nourishing the herb-eating animals as that which the animal fibrin and albumin play in nourishing the flesh-eaters. It follows that the growth and development of all animals are dependent on their receiving certain substances from the plants identical, or nearly identical, in composition and in their

general properties with the chief constituents of the blood of the animals themselves.

In this sense, Liebig taught that the animal organism gives to the blood only its form, that it is incapable of creating blood except out of substances which already contain its chief constituents, in the form of compounds closely allied to those which occur in the blood itself. Liebig did not mean that the animal organism cannot produce new compounds from its food, for it was well known that an extensive variety of compounds do result from the life processes of animals, but that the organism depends for its starting-point on certain substances in the blood which are so much like the albuminoids of plants that at Liebig's time they were practically indistinguishable, so that as far as the albuminoids are concerned, the development of the animal begins where the life of the plant ends. From this point of view, the plant holds an intermediate position between the mineral and animal worlds. The animal is incapable of assimilating the compounds stored up in inorganic nature. To render them fit for the support of the animal, they must undergo a course of preparation in the plant, in which process the simple stable molecules of the mineral substances are converted into more complex and less stable molecules of a higher order, from which may afterwards be built up the yet more complex and yet more instable substances which are capable of doing service in forming and maintaining the life of the animal.

The main pillar of this great generalisation, which we owe to the work of Liebig and Mulder, consists in the fact just quoted—viz. in the identity, or, to be more correct, in the close chemical similarity between

the nitrogenous compounds found in plants and those which occur in the blood of animals.

Liebig illustrated the importance of the albuminous substances to animal life by calling attention to the changes in which a chick develops from an egg. Both the white of egg and the yolk are largely composed of albuminoids. An egg, after impregnation, if maintained at a suitable temperature, with the aid of the oxygen of the air, which finds ready access through the porous shell, gradually develops all the parts of the animal body—feathers, claws, membranes, fibrin, blood-vessels, nerves, and so on. In the process, all the albumin disappears. Evidently albumin is the foundation, he said, of the whole series of peculiar tissues which constitute those organs which are the seat of vital actions. The elements of these organs, which now possess form and vitality, were originally elements of albumin.

The results of examining other alimentary substances always told the same tale; he found albumin—or, at least, those bodies closely allied to it, which I have called albuminoids—present in every food, which by itself suffices to support animal life.

What we call "meat"—that is, the muscle of herbivora—is very largely composed of solid albuminoids.

If we examine milk, the food prepared in the body of the mother for the nourishment of her young, we shall find in it, besides a kind of sugar, fat, and a little albumin, a substance called casein. Now casein is nearly identical in composition with the albuminous constituents of blood fibrin and albumin, and also analogous in its nature to the vegetable casein found in peas and beans. The

young animal, therefore, receives in this form the albuminous material, which provides it with the fundamental constituents of its blood, from which its flesh, bones, and nerves must be elaborated.

Again, eggs, which are so rich in albuminous matter, in a form very favourable to its complete absorption, will, even by themselves, support life; and cheese, which contains both the fat and the casein of milk, would doubtless do still better if it were equally digestible; but a diet of fat, let us say of butter or of starch, or of butter and starch together, soon leads to starvation. This we can well understand aided by the light which Liebig has thrown on the subject. Fat and starch are non-albuminous substances; albuminous material must be provided in our food to secure the production of blood, and therefore blood cannot be formed from a diet consisting of fat and starch alone. That is why these foods will not by themselves support life.

Albuminous material, then, must be present in the food of animals; *they cannot produce it for themselves*, though they may modify it; they depend upon the plants for a supply of it, and those parts of plants which contain a good proportion of albuminous matter will, if digestible, generally be found more nutritive than those which contain very little.

In the "Familiar Letters on Chemistry" (p. 350) Liebig summed up his views in 1851 in the following oft-quoted passage:—

"How admirably simple, after we have acquired a knowledge of this relation between plants and animals, appears to us the process of formation of the animal body, the origin of its blood and of its organs! The vegetable substances which serve for the production

of blood contain already the chief constituent of blood ready formed, with all its elements. The nutritive power of vegetable food is directly proportional to the amount of these sanguigenous compounds in it; and, in consuming such food, the herbivorous animal receives the very same substances which, in flesh, support the life of the carnivora.

"From carbonic acid, water, and ammonia—that is, from the constituents of the atmosphere—with the addition of sulphur and of certain constituents of the crust of the earth, plants produce the blood of animals; for the carnivora consume, in the blood and flesh of the herbivora, strictly speaking, only the vegetable substances on which the latter have fed. These nitrogenised and sulphurised vegetable products, the albuminous or sanguigenous bodies, assume, in the stomach of the herbivora, the same form and properties as the fibrin of flesh and animal albumin do in the stomach of the carnivora. Animal food contains the nutritive constituents of plants stored up in a concentrated form.

"A comprehensive natural law connects the development of the organs of an animal, their growth and increase in bulk, with the reception of certain substances, essentially identical with the chief constituents of its blood. It is obvious that the animal organism produces its blood only in regard to the form of that fluid, and that nature has denied to it the power of creating blood out of any other substances, save such as are identical in all essential points with albumin, the chief constituent of the blood.

"The animal body is a higher organism, the development of which begins with those substances

with the production of which the life of those vegetables ends which are commonly used for food. The various kinds of grain and of plants used for fodder die as soon as they have produced seeds. Even in perennial plants a period of their existence terminates with the production of their fruit. In the infinite series of organic products which begins with the inorganic food of plants, and extends to the most complex constituents of the nervous system and brain of animals, the highest in the scale, we see no blank, no interruption. The nutritive part of the food of animals, that from which the chief material of their blood is formed, is the last product of the productive energy of vegetables."

The vegetable matters which the graminivorous animals consume are not, however, entirely, or even chiefly, composed of albuminous substances, and in some of them the proportion of albuminoids is very small.

The non-nitrogenous components of vegetables may be divided into mineral matter—the carbohydrates—which occur in relatively large quantities almost invariably, and fats, which are usually present in small or decidedly moderate proportions. It is found that a large part of these organic materials can be absorbed and made useful by animals; the fibre of vegetables is to a great extent rejected, however, especially the coarser parts, but such components as starch and sugar are readily absorbed, and are very useful. The carbohydrates and the fats contain only carbon, hydrogen, and oxygen; they contain no nitrogen, no sulphur, no phosphorus.

The food of the carnivora also contains a certain amount of non-albuminous matter. Flesh contains a

variable quantity of fat, and milk also contains fat (from which comes butter), and with it a crystallisable substance called sugar of milk, which belongs to the class of compounds called the carbohydrates. (*See* p. 85.)

The relative proportions in which the albuminoids, fat, and sugar occur in milk are not constant, and similarly the proportions which albuminoids bear to the other constituents of flesh and of vegetables vary considerably in different specimens. Thus oats, on an average, contain about 65 per cent. of carbohydrates and fibre, and about 12 per cent. of albuminoids, but a given sample of oats would probably be found to have rather more or rather less than this proportion of albuminoids or of carbohydrates.

These facts lead us to Liebig's celebrated classification of the food of men and animals. "The food of men and animals," he said, "consists of two classes of substances essentially different in their composition."

"The one class, consisting of nitrogenous substances, albumin, etc., serves in the formation of blood and in building up the various organs of the body; it is called the *plastic food*. The other, consisting of non-nitrogenous substances, the fatty bodies, and the so-called carbohydrates, resembles ordinary fuel, serving as it does for the generation of heat; it is designated by the term *respiratory food*. . . . We heat our body, exactly as we heat a stove, with fuel which, containing the same elements as wood and coal, differs essentially from the latter substance in being soluble in the juices of the body."

In order to establish this classification of foods, he

pointed out that the food of all animals contains, as we have seen, besides the plastic or sanguigenous constituents from which the blood and the organs are derived, a certain amount of the substances which contain only carbon, hydrogen, and oxygen. By making various mixtures of articles of food, a diet can be obtained which, so far as its composition is concerned, will suit the needs of any given man or animal, and in making such mixtures a healthy man is guided by an instinct which prescribes for him the best proportions in which to mix the plastic and non-nitrogenous materials for his diet. These proportions may be altered within certain limits, which again vary with the individual, his mode of life, state of health, etc., and may even be varied beyond those limits, under compulsion, without at once involving the death of the individual; but such alterations are never made without more or less injury to the bodily and mental powers of the individual concerned. In no case can life be for long maintained if the proportion of the nitrogenous constituents be reduced below a certain fixed minimum. And though, on the other hand, it is possible to maintain life on a purely albuminous diet, it is always disadvantageous to do so.

Guided by what was then known of the composition of the body and of the food-stuffs, Liebig concluded that it was demonstrated that different foods are exceedingly unequal in their ability to influence the producing and restoring of the powers which enable men and animals to do work or produce manifestations of energy through the nervous system: that wheat surpasses rye, that rye surpasses potatoes, and that flesh surpasses all other foods in respect to the production of these effects. Now, the proportion of the plastic to the

non-nitrogenous materials (taking Liebig's data) in the case of potatoes is smaller than in the case of rye, that in the case of rye is smaller than that in the case of wheat, and that in the case of wheat again is very much smaller than that in beef or mutton, from which it seemed obvious that the plastic constituents of food must be the proximate cause of the power to do work and of the nervous manifestations in the animal organism. This conclusion is confirmed by the fact that all the effects which animals produce by their brains or by their muscles are determined by the organised structure of their bodies, and that the unorganised parts, such as fats, are unable to change their relative positions by any inherent power of their own.

But if the effects producible by a man or beast, whether by the voluntary or involuntary motions of his muscles or by the organs of sense, depend upon the producing and restoring of the organised tissues, if these organised tissues are built up and renewed by albuminoid material derived from the blood, and if finally the albuminoids of the blood find their origin in the albuminoid constituents of plants, it seems to follow that in these albuminoid constituents of plants we have the source of all those effects which animals produce by means of their organs of sense, thought, or motion.

Liebig believed that in this relation of the animal kingdom to the vegetable kingdom he had found a new and wonderful revelation. "Plants, which serve as food to animals, are the producers of the plastic constituents of food, and hence are accumulators of force." *

* We should now say "energy."

By becoming organised parts of the animal body the plastic constituents of plants determine, according to Liebig, the continuance of all vital phenomena.

But what, then, is the function of the non-nitrogenous food? Liebig found in this the source of animal heat.

The animal body is not, he said, merely a source of mechanical power and of vital actions; it is also an apparatus for producing heat; sufficient heat is given off in a year by an adult man to raise about twenty-three thousand pounds of water from freezing-point to boiling-point. This heat is the outcome of the combination of the oxygen of the air, taken up in respiration, with the constituents of the food in the body, and the amount of heat produced is approximately in proportion to the oxygen consumed. On comparing the consumption of plastic material with the oxygen taken up by the lungs, Liebig found that the combustible elements of the former were insufficient to convert all the oxygen absorbed into carbonic acid gas and water—that a horse, for example, takes up about five times as much oxygen as is wanted for the complete combustion of the albuminoid substances in the food. For this and other reasons he concluded that the non-nitrogenous substances in the food supply the blood with materials which surpass the albuminoid compounds in their tendency to undergo oxidation, and thus protect, as it were, the albuminoids from the destructive action of the oxygen. This is the function, according to Liebig, of the non-nitrogenous part of food.

"Sugar, starch, and fat," he says, "serve to protect the organised tissues: and, in consequence of the

combination of their elements with oxygen, they keep up the temperature of the body."

"The sulphurised and nitrogenous constituents of food determine the continuance of the manifestations of force; the non-nitrogenous serve to produce heat. The former are the builders of organs and organised structures and the producers of force, the latter support the respiratory process; they are the materials for respiration."

The two quotations given above are taken from the "Familiar Letters" (1851). But the same idea, very clearly expressed, is to be found in Part I. of his second "Report to the British Association," presented in 1842.

This beautiful conception quickly won its way, and for a long time it was generally accepted. It was, in a word, a great success; but, like all other classifications, it was imperfect. It is difficult to draw the line which will exactly separate the plastic foods from those which only support respiration, and physiology now teaches us that all varieties of food are both assimilable and respiratory. But the value of a conception of this kind does not depend on its actual and final truthfulness: it depends on its usefulness. A theory, as Liebig always taught, is only a tool. It is only valuable if we can employ it for the purpose of extending the bounds of knowledge. Sooner or later its work, like that of other tools, will be done, and it will be cast aside. Liebig's theory of the plastic and respiratory foods, though now, at last, no longer serviceable, in its day did yeoman's service. Even to-day it is found convenient to retain Liebig's classification of foods into the nitrogenous and non-nitrogenous groups; for it is still true, and always

will be, that the members of the two groups exhibit marked differences in their effect on the animal organism. The nitrogenous constituents of food alone are by themselves capable, though not usually advantageously, of sustaining animal life. They alone contain all the elements that are required both for building up the tissues and for supporting the respiratory processes. The non-nitrogenous constituents, on the other hand, by themselves are quite incapable of sustaining life. They do not contain the necessary elements for forming the organised tissues; they only supply those elements which by their oxidation are capable of serving as sources of heat and work. In a modified form this is the distinction that was drawn by Liebig, and on which he founded his theory that the use of the non-nitrogenous parts of food is to protect, as it were, the nitrogenous constituents. Before Liebig, although physiologists had experimented with various simple foods, and had learned whether this, that, and the other substances were suitable for the use of animals, their results had led to no general conclusions. They had not recognised, as Liebig did, that all foods fall into two large and fundamentally distinct classes. It was this, perhaps, which led Dr. Theodor von Bischoff to declare, in 1874, that though the objections which had been raised on physiological grounds against Liebig's classification were undoubtedly correct, yet the truth of his views as a whole would always remain, and that it was impossible to deny the great merit they possess of pointing out in the briefest way the essential differences which separate the several varieties of food.

It will be remembered that Liebig's views on the

relations of plants and animals led him to declare that through their nitrogenous constituents the plants act as accumulators of energy. This brilliant idea was also in the main correct. The advance of knowledge has shown us that plants do indeed store up energy—not, however, only in the processes by which their nitrogenous parts are formed, but, speaking broadly, by their life processes as a whole, in which, as we now believe, energy derived from the sun is made use of for separating the carbon and oxygen from the carbonic acid gas of the air. This energy remains, stored up as it were, in the new substances formed and in the liberated oxygen, and again becomes available on the recombination of the carbon and oxygen either in our bodies or under other conditions.

Another conclusion of Liebig's which led to a most animated discussion, was his doctrine of the origin and functions of fat in the bodies of animals.

By a careful investigation of the conditions under which fat appears in animals he was led to conclude, positively, that fat is actually elaborated by the organism; that it is not all taken in as fat in the food, but is partly produced from the carbohydrates which the animal finds ready prepared for him by the plants. The hypotheses by which he sought to explain this phenomenon are no longer acceptable, but the fundamental fact of the production of fat by animals has been abundantly confirmed. When Liebig first announced his conviction that fat is produced in the bodies of animals from the carbohydrates, his decision was vigorously criticised by some of the leading French chemists, who were already committed to the opposite view. About a year before Liebig's Report was

published—viz. on August 20th, 1841—Dumas and Boussingault, in their celebrated lecture on the "*Statique Chimique des Êtres Organisés*," had declared that plants produce fat, and that animals consume it; and these two especially threw themselves into the debate on this subject, which soon became lively, and was not quicky concluded. Liebig stated that the food consumed by the herb-eating animals does not contain anything like as much fat as that which they store up in their bodies. His opponents, on the other hand, held that the animals receive all their fat from the plants. Curiously enough, Liebig was shown to be right, largely by the researches of those who opposed him.—This sometimes happens in scientific disputes, and had already occurred sufficiently often to Liebig to lead him to say on one occasion, "My mill has ever received its best supply of water from my opponents."—Their experiments showed, indeed, that the vegetable food of animals contains more fat than had previously been supposed, but that in test cases the amount is quite insufficient to account for the quantities that are deposited in the bodies of pigs and geese when they are given a vegetable diet. The question was finally settled when it was proved that bees produce wax, which Brodie has shown to be a fat, when they are fed exclusively upon sugar.

The work of the French chemists yielded, however, a further interesting result. It showed that this producing of fat from the carbohydrates was largely dependent on the co-operation of nitrogenous foods. This Liebig admitted, and also that under certain conditions the nitrogenous materials themselves might become the source of supplies of fatty matter.

At a later stage some modern physiologists re-opened this question, and, going beyond what Liebig was able to admit concerning the producing of fat from the albuminoids, assumed that all the fat produced in the animal comes from this source. Liebig, without denying that this view might be right, held that all that could be said with certainty was that in the case of the graminivora the albuminoids and carbohydrates work together to produce fat; and the results of subsequent experiments have, it may fairly be said, vindicated his opinion on this subject.

So much has been said about the classification of foods into nitrogenous and non-nitrogenous, that it has become important to remind the reader that neither nitrogenous food alone, nor non-nitrogenous food alone, nor both together, will support life. Organic foods—that is to say, those which contain carbon, hydrogen, oxygen, nitrogen, sulphur, and phosphorus—do not by themselves include all the materials present in the animal body; and it is said that if a diet composed of purely organic materials be offered day after day to an animal, he will, after a time, refuse to take it—that not even the tortures of starvation will induce him to do so.

If the body of a dead animal, or a portion from almost any part of the body of an animal, be burnt as completely as possible, there will always remain a more or less voluminous residue of ash. This ash or mineral matter is as essential to the life of an animal as the organic matter, and it must, therefore, be provided in his food. At one time the importance of mineral matter both in the food of plants and animals was to a great extent overlooked, and we have seen

that even comparatively lately it was supposed that organisms could produce mineral matter for themselves. The importance of mineral matter for the bones of the higher animals was of course recognised, but that was all.

Liebig, as has been seen in the earlier sections of this book, was pre-eminent among vegetable physiologists in regard to the importance he ascribed to the mineral food of plants ; naturally, therefore, he did not overlook its vital significance in the animal economy. When we have said, however, that mineral food is of vital importance to animals, that nevertheless it does not serve as an important source of energy, and have added a few facts, such as that the bones contain phosphate of calcium, that bile contains much sodium, chiefly in combination with certain peculiar acids, we have told almost all that is known. In fact, this subject, important as it is, is still imperfectly understood, and only merits this brief mention, in order that the preceding pages shall not mislead unscientific readers by creating the idea that it is only the organic constituents of foods which are important.

In the second part of his second report to the British Association Liebig attempted to trace the changes that occur during the processes which convert the constituents of food into blood, those of blood into tissues, and, finally, these into the secretions and excretions.

The value and interest of most of this splendid review of physiology, from the standpoint of the chemist, has now necessarily to a great extent passed away, in consequence of the transformation which the subject has undergone in the hands of a host of modern

workers. Liebig himself, there can be no doubt, foresaw that to a great extent his conclusions in this department would in time be modified. He admitted frankly that some of his results had surprised himself no less than they would surprise others; that they had excited in his own mind the same doubts which others would entertain; that he gave them because he was convinced that the method by which they were obtained was the only one from which an insight into organic processes could be hoped for. He felt that they deserved attention in so far as they pointed to the road which chemists must follow if they would be really of service to physiology and pathology.

By showing how to apply chemistry to physiology, Liebig insured, first, the advance of physiology, and, secondly, the overthrow of many of the details of his own contributions to its advancement. This second effect of his work may be illustrated by an example drawn from his treatment of the metamorphoses of the tissues alluded to above.

The basis of many of Liebig's early conceptions concerning the nature of the tissues was the supposed existence of the substance called *protein*, which had been obtained by the physiologist Mulder from albuminoids by the action of caustic potash. When the progress of physiological investigation showed, as it subsequently did, that the protein of Mulder was, so to speak, an artificial substance, a mere product of the laboratory and no true component of the tissues at all, all those hypotheses of Liebig's which were based upon the protein theory fell to the ground. But the very fact that they did not survive, but were overthrown in this way to make room for truer con-

ceptions, was itself a testimony of the highest kind to the immense value of his method, since the very progress which led to the overthrow of these particular conclusions was itself, in no small degree, the outcome of the method which Liebig had given to physiology.

It is worth while to lay emphasis on this matter, for, important as Liebig's contributions to the advance of physiology were, they are to-day to a great extent forgotten. This was the case even twenty years ago, when Theodor von Bischoff said on the subject: "I do not think I am mistaken if I hold the opinion that there are not many among the younger physiologists and medical men who know, or have even a distant notion how great—or, rather, I should say, how immense—the influence of Liebig's researches, writings, and teachings has been, and is still, not only on physiology and medicine, but on organic science at large. The majority enjoy the advantages gained, and rejoice in the progress, without being aware of their author. They consider as self-evident the facts established by Liebig and the methods and principles of research diffused by his teaching. They believe it could not be otherwise, and care but little for him to whom science, and with science they themselves, are indebted for their present position."

The man who wrote these words was not only himself an eminent biologist, but he had known Liebig and his work for thirty years. He was one who knew the condition in which Liebig found physiology and how he left it. On the whole, it seems that what Liebig especially did for physiology was this: He taught the physiologists of his time how to frame many questions, and how to search for their answers.

In the third section of this Report Liebig dealt with the phenomena of motion in the animal body. He recognised a connection between the oxygen absorbed and the work done, and he perceived that an increased transformation of muscular fibre determines the doing of a greater amount of work. He perceived, in short, an intimate relation in the animal body between absorption of oxygen, change of matter, and manifestations of energy. But he was too early in this field, and, besides, the problem was one for a physicist rather than for a chemist. The thought he gave to the subject was, however, by no means unproductive. In connection with it he attacked a question of the greatest practical importance in regard to the feeding of animals for work—viz. whether an animal when at work requires its food to be enriched with the nitrogenous elements, or whether the increased demands are rather upon the non-nitrogenous constituents. The answer to this question for some time seemed doubtful, but it now appears to be pretty clearly proved that, contrary to Liebig's opinion, under ordinary circumstances the enlarged requirements of the animal are chiefly for the non-nitrogenous foods.

The final solution of the question has only been arrived at by following a line of investigation suggested by Liebig's writings.

Practically speaking, we may say that just as the carbon of the food is ejected from the animal system through the lungs in the form of carbonic acid gas after its work is done, so the nitrogen is mainly—though not, indeed, solely—ejected by the higher animals in the form of a single substance, urea. It follows from this that if we can in any way collect day

by day the urea which passes from an animal and weigh it, we shall have an approximate measure of the amount of nitrogenous tissue used up during separate periods. Further, if we divide the life of a given animal into periods of work and periods of rest, and compare the quantities of urea thrown off during those periods respectively, we shall be able to tell whether increased demands have been made on the nitrogenous matters of the tissues during the working periods; because in that case we shall find that more urea is produced during or immediately after work.

In order to trace the influence of work on the tissues by periodically measuring the urea produced it is necessary to devise some means of readily separating the urea from the substances which accompany it, and then to measure it. This was accomplished by Liebig by taking advantage of the fact, that if a solution of corrosive sublimate be added to urea in solution, these two combine and separate as a precipitate of definite composition. The quantity of the urea in a solution may be determined either by collecting and weighing the precipitate—which is troublesome—or, more simply, as Liebig showed, by adding a solution of corrosive sublimate of known strength to the solution of urea, so long as any precipitation occurs, and then calculating from the amount of sublimate employed how much urea was in the solution to be tested.

This process, though superseded of late years, has been of immense value, not only to the student of pure science in the study of the transformations of the tissues in the body, but also in the hands of physicians, by enabling them to watch the action

of the organ (the kidney) by means of which urea is, under normal conditions, absorbed from the blood, and so removed from the system, and to detect and treat certain diseases. Liebig and his pupils also enriched physiology by a chemical examination of blood, by a study of the nature of bile, and by comparative work on the elimination of the waste products of man and the carnivora. Jointly with Wöhler he conducted a research on uric acid, which is justly regarded as a model of experimental inquiry. In these contributions to science, however, though Liebig showed his wonderful powers as an investigator, he was not touching any of the fundamental questions, and we must not, therefore, dwell upon their details, but pass on to his inquiry into "the motion of the juices in the animal body,"* in which he suggested some further extensions of the applications of chemical and physical laws to physiology.

Nourishment produced from the food is, we know, carried to every part of the body by the blood. It travels through tubes—some of them large, some of them very small indeed—which extend by their ramifications throughout the whole organism. But these tubes necessarily have walls, though some of them are very thin, and the tissue which they supply, however well it may be provided with blood-vessels, cannot all of it, or even most of it, be in actual contact with a blood-vessel.

The question, then, is—How does the nourishment find its way from the blood, through the walls of the vessels and cells, to the points where it is wanted? And, again, how do the products of the never-ceasing

* "Chemistry of Food," by Dr. Gregory, published 1848.

changes that go on in the tissues find their way through the same obstacles and into the blood which is to carry them away when their work is done?

If we take a bag, or closed tube made of parchment, of bladder or of any similar membrane, and put in it alcohol or a solution of common salt, and if we then put the closed end of this under distilled water in a basin, we shall soon find that the water in the outer vessel is no longer pure. It will contain alcohol, or salt, as the case may be; for these things, and many others, when dissolved, have the power of passing through such membranes. The name *osmose* has been given to the process by which this transference of solids in solution through membranes takes place; and it has been supposed that nourishment is conveyed to the living tissues by a process of this kind.

From the results of a number of experiments, some made by himself, others by other investigators, Liebig formed the opinion, that in the organism the causes which determine the motions of the juices are far more powerful than that to which the name osmose has been given. That the passage of the digested food into the blood, the passage of the nutrient fluid outwards from the blood, and its motion towards the parts where its constituents acquire vitality, involve something more than a simple law of mixture. He concluded that the membranes probably exert an important influence on the secretions. The following quotation will perhaps indicate the position he took as well as a more detailed account of his work:—

"Since," he says, "the chemical nature and the mechanical character of membranes and skins exert the greatest influence on the distribution of the fluids

in the animal body, the relations of each membrane presenting any peculiarity of structure, or of the different glands and systems of vessels, deserve to be investigated by careful experiment; and it might very likely be found that in the secretion of the milk, the bile, the urine, the sweat, etc., the membranes and cell-walls play a far more important part than we are inclined to ascribe to them; that, besides their physical properties, they possess certain chemical properties by which they are enabled to produce decompositions and combinations, true analyses; and if this were ascertained, the influence of chemical agents, of remedies, and of poisons on those properties would be at once explained."

It is nearly fifty years since these words were written, and the subject is still, owing to its difficulty, not far from where Liebig left it, except that the modern physiologist would remind us that osmose in a living membrane is probably very different from that in a dead membrane. But is not this just what Liebig intended to point out when he suggested that the membranes and cell-walls play a specially important part in the phenomena in question? Sooner or later Liebig's suggestions will probably bear fruit, and, indeed, an interesting step in the direction indicated by the above remarks has been made by Professor Waymouth Reid within the last few years. Professor Reid, by means of experiments with a frog's skin, has found that in the living membrane there is a distinct absorptive force which makes osmose take place more readily from without inwards than in the reverse direction.

A year before the publication of this account of his experiments on osmose Liebig produced, in his

researches on the chemistry of food, an account of his investigation of the constituents of flesh.

The introductory part of this little book illustrates rather forcibly Liebig's power of giving "hard knocks." In his own work he had often shown the good use that may be made of an exact knowledge of the chemical composition of substances. But he now found it necessary to remind the physiologists that the function of chemistry in regard to physiology is by no means exhausted when a substance is analysed and neatly labelled with a formula more or less correct, especially as the most exact analyses cannot by themselves always lead us to true formulæ.

It seems that the discovery of a close similarity in composition between casein, albumin, and fibrin, from animal and vegetable sources, which appeared to throw much light on the processes of digestion and nutrition, had, in Liebig's opinion, created an undue sense of the importance of the mere chemical composition of organic substances, with the result, as he said, that for some time a great many analyses had been made, but not much real progress. This led him to give a new example, drawn from one of his own most brilliant investigations, of the true manner in which organic chemistry should be applied to the investigation of physiological problems.

There are two reasons why the mere analyses of the substances which occur in animal bodies cannot by themselves considerably advance physiology. First, that an organic analysis is, as he reminded his readers, a means of acquiring knowledge, but is not that knowledge itself. The results of an analysis, however perfect it may be, do not give the least information as to the arrangement of the elements

in the molecules of the substances, nor as to how they will behave themselves under the influence of chemical agencies; and till we know this we cannot possibly form any definite views of the part a given compound will play in the vital processes.

Secondly, it is only when the number of atoms in the molecules of a compound is small that an analysis can be trusted even to lead to a correct formula; and as the molecules of the substances dealt with in physiological chemistry often are large, small inevitable errors in their analyses may, and do, lead to very serious misconceptions.

There is, however, a method, which was well known to a master like Liebig, by which we can both check the result of an analysis and simultaneously gain some knowledge of the arrangement of the atoms in the molecules of an organic substance. This method is simple enough in principle. It may be described as an application of the maxim "divide to conquer." It consists in this, that when the composition of a substance has been found as accurately as is possible by analysis, and its most probable formula has been provisionally fixed upon, portions of it are treated with reagents, so that, if possible, its molecules shall be broken up into two or more substances with less complex molecules. These more simple compounds are then in their turn submitted to analysis, and the results so arrived at are compared with those given by the substance from which they were derived.

For each organic substance there may be several formulæ which agree almost equally well with the results of its analysis, but probably only one of

these will also agree with the formulae deduced from the results of the analyses of the substances derived from it.

The importance of this method of control had not, it is true, been entirely overlooked by workers in the field of physiology: but it is subject to sources of error, which had not been properly guarded against. In order that this method shall guide the investigator to true results, the decomposition products must every one of them be fully and thoroughly investigated. If one of them be missed, for example, an error results which may mislead the investigator and his followers, with most deplorable results.

Above all, hypothetical compounds must not be assumed to exist among the products oi the decomposition in order to support preconceived ideas, or to save trouble.

These are words which might well be written in letters of gold over the door of every laboratory for chemical research, and Liebig, by insisting on the importance of the principle thus laid down, did a distinct service to the sciences which it was ever his object to advance.

In the second section of the book Liebig dealt with his discovery of the formula of creatine, and with his other important discoveries concerning the components of flesh. His method in this work afforded to the chemist and physiologist an admirable illustration of the principles laid down in the preceding paragraphs; but the subject is of too technical a character to be generally interesting in spite of the important results he obtained.

In the last section of his "Chemistry of Food" Liebig dealt with the chemistry of cookery, so far as

concerns the use of flesh. It is obvious, he pointed out, that if the flesh employed for food is to become again flesh in the animal body, none of the constituents of raw flesh ought to be withdrawn from it in its preparation for the table. If any essential constituent be removed from a piece of flesh, that piece of flesh will no longer have the power of resuming in the body the form and quality on which its properties in a living organism depend. But when flesh is boiled in water, more or less of its saline constituents are removed, together with a certain amount of its organic constituents. It follows that boiled meat eaten without the liquor in which it was boiled is less well adapted for nourishing an animal, in proportion to the amount of matter that has been extracted from it—in proportion, that is to say, to the length of time during which the process of boiling is continued and to the quantity of water employed.

It is possible to extract from finely divided meat, by the action of cold water, a great deal of albuminous matter—the proportion varies with the age of the animal and other circumstances—hence it is not wise to steep fresh meat for long in cold or warm water. On the other hand, washed muscular fibre becomes hardened by *boiling* it in water, consequently the tenderness of boiled meat depends largely upon the cook's success in depositing the soluble albuminous matter coagulated among the fibres, since this prevents to some extent the contracting or hardening mentioned above. The method to be recommended, therefore, for boiling meat, especially when the water in which it is boiled will not be consumed, is to plunge the cold meat into *boiling water*, then after a few minutes to cool the water to from 76 degrees to

72 degrees,* and to maintain this temperature for a sufficient time.

It is easy to understand that the effect of the brief immersion in boiling water is to form an outer case of hardened flesh, which protects the mass beneath from the solvent action of the water afterwards. Then the long-continued moderate heating gradually warms the whole to a sufficient temperature to coagulate the albumin in the juice of the meat without unduly hardening it. The colour of underdone meat, as Liebig explained, is due to the mass having been insufficiently heated. The albumin of meat juice congeals at about 60° C.,† but the colouring matter of the blood requires a higher temperature. That is why the meat should be heated to at least 72 degrees. Finally, a rather lower temperature may be employed for white meat, such as the flesh of fowls, as this contains little blood. This last fact explains why such meat is more quickly cooked than beef and mutton.

The superior flavour of roast meat, as will now be seen, is largely due to the fact that in cooking it nothing has been washed out of it; besides this, the retained albumin reduces hardening of the fibres to the utmost possible extent. But even in roasting, the escape of the juices should be retarded by heating as strongly as possible at first; the juice then hardens on the outside and produces a protecting surface, which prevents subsequent loss.

In order to prepare soup we must, as far as possible, reverse all the above proceedings, and conduct the operations so as to extract as much as

* 165 to 158 degrees Fahr.

† 140 degrees Fahr.

possible from the meat. For this purpose, therefore, Liebig advised that the meat should be very finely divided, and then be brought into cold water and heated *very gradually* to the boiling-point, at which it should be kept for a few minutes.

Soup made in this way may not form a jelly on cooling, but it will be very rich in the most valuable of the soluble parts of meat. Housewives have always, and probably do still, attach great importance to the tendency of soup to form a jelly. Liebig insisted that the importance of this quality is much overrated.

As the high temperature Liebig employed at the end of his process may coagulate albumin, a modification of it has been suggested in which the finely divided meat is placed in cold water and then gradually heated as before, but not to so high a temperature. Thus we avoid coagulating the albuminous substances dissolved at the lower temperature, which are consequently retained in solution.

A few words must now be said about Liebig's celebrated extract of meat.

By boiling down extract of lean flesh, made by Liebig's process, at a low temperature in a water-bath, or by one of the other methods of low-temperature evaporation which are at the command of chemists, we obtain "Liebig's extract of meat." In 1847 Liebig did not lay so much stress on the usefulness of this product as he did afterwards. In fact, at that time he only recommended it as capable of making strong, well-flavoured soup, and as likely to prove a valuable restorative to wounded soldiers.

In his celebrated "Familiar Letters on Chemistry" Liebig again dealt with this subject, and recommended

soup in the strongest terms as the medicine of the convalescent. He here pointed out that in South America, in Mexico, and in Australia, where animals are chiefly valuable for their skins and wool, there might be made from meat of only nominal value * immense quantities of the best extract, which might acquire great importance for potato-eating populations, and replace soup in the hospitals. In these letters he also discussed more fully the question of the dietetic value of gelatine, and of soups containing it, quoting the report of a commission of the French Academy, with Magendie at its head, to show that the value of a soup is very little increased by the addition to it of gelatine, since numerous experiments have proved that by itself gelatine has little or no dietetic value—at any rate, that it cannot be made to act as a substitute for a true plastic food, in spite of the nitrogen which it contains.

The extraordinary and rapid rise into popularity of the condensed extract of meat was remarkable, and it was made more wonderful by the fact that for a long time a most marked want of agreement existed as to its real value. Whilst some people regarded it as highly unwholesome, or, indeed, almost a poison, owing to the large proportion of potash salts in it, others—some of them, however, from interested motives, it is to be feared—went so far as to recommend it as a really sufficient substitute for meat: and, more wonderful still, a great many others believed them. These last, however, were, perhaps, to be excused. Ignorant of the most elementary

* Lean beef in Australia about this time was worth from one halfpenny the pound to nothing; great quantities were thrown away after boiling down for the fat.

principles of science as the general population was then, and still is, and familiar as they were with the striking, and to them quite unintelligible, results that often follow the administering of apparently trivial quantities of active chemicals, such as quinine or arsenic, it is not, after all, really surprising that many people were ready to believe almost anything they were told about this wonderful invention of the great German chemist.

Liebig himself was not likely to be, and never was, under any mistake about the value of extract of meat. At first it would appear from his chemistry of food that he regarded it as a valuable restorative, and as a substitute for soup. Afterwards he perceived that it might have a wider application, and become the means of bringing to Europe some part of all the food that was wasting at a distance literally for the want of anyone to eat it. But he meant the extract to be eaten with liberal additions of bread, peas, lentils—with food, that is, which contains a good supply of those nitrogenised constituents in which the meat extract was, from the process of its manufacture, intentionally and of necessity wanting. It is still difficult to understand its exact action; but the tendency is to regard it as a useful stimulant, ranking, perhaps, with tea and coffee, but, of course, with its own peculiar virtues. Bischoff has pointed out that the salts in extract of meat are probably just those which are most wanted for the producing of flesh, though, of course, salt must be added; but as nitrogenous matters of vegetable origin are inferior, for the food of carnivora, to those obtained by means of flesh, he concluded that a diet in which the vegetable albuminoids replace those of flesh must be inferior to flesh food,

even when supplemented by meat extract. Liebig, however, as Bischoff has said, by no means overlooked this, and never recommended the extract as equivalent to flesh, but only as a substitute for it when taken with suitable accompaniments.

In concluding his contribution to domestic chemistry—so far as the treatment of flesh is concerned—Liebig turned to the processes of salting and preserving meat. By collecting the brine from a given piece of flesh which had been pickled, and measuring it, he found that it corresponded to about a third of the juice of the given flesh; and on subjecting this brine to analysis Liebig was interested to discover that it contained the constituents of soup, which had been extracted to a greater extent than can be done in the ordinary way, even by boiling water. Not only the salts, but a large quantity of albumin, together with creatinine, etc., were found in it. These striking observations were of the greatest practical interest. They explained why salt meat affords a less valuable food than the same meat unsalted; why, in the days when salt meat had to be largely employed for food during the winter, our countrymen, in spite of the other conditions of their lives being in many ways favourable, were scourged by various diseases from which we ourselves are comparatively free; and why, even in modern times, toilers on the sea, when they are driven to subsist for a long while on salt meat, often suffer for it severely. In the course of his experiments on salting meat Liebig made the further interesting observation that the presence in the salt of calcium or magnesium compounds, in some degree protects flesh from its extractive power. It seems as though the salts of these metals, on

meeting with the soluble phosphates of the flesh, produce insoluble substances by interacting with them, viz. phosphate of calcium, or of magnesium, which cannot be so freely removed from the flesh as the more soluble alkaline phosphates themselves; and he suggested that meat pickled with salt containing these metals would, if eaten with a sufficiency of the esculent vegetables, which are rich in potash, offer a very superior diet to ordinary salted meat.

One of the most interesting problems connected with physiology is the question whether the life processes of living organisms are determined by the action of a so-called "vital force."

It is not possible, when we think upon the wonderful results that flowed from Liebig's endeavours to apply the methods of organic chemistry to physiology, to avoid asking ourselves the question—Did he, then, consider that in the laws of chemistry and physics we have a key which may ultimately unlock the chamber which hides the mystery of the origin of life? Did he suppose that the laws of chemistry and physics were the sole laws of physiology? We know that he, like Davy and some others, rejected altogether the idea that there exists a peculiar vital force which is able to override the laws of chemistry—as, for example, by the creation of mineral substances from the inorganic components of their food—and which presides unassisted over the phenomena exhibited by organised beings. But did he go so far as to reject the very idea of the existence of any vital force, as some thinkers have been inclined to do? It would be easy to find passages in his writings which seem to imply that this was the case. "It is evident," he

says in Chapter XX. of his Familiar Letters,* "that physiology has two foundations, and that by the fusion of physiological physics, the foundation of which is anatomy, with physiological chemistry, which rests on animal chemistry, a new science must arise, a true physiology, which will stand in the same relation to the physiology of the present day as modern chemistry does to that of the eighteenth century."

Then, after showing how chemistry had in the past absorbed and made part of itself branches of physics, with the result that the latter had determined the whole character of modern chemistry, he goes on to say that—"Exactly in the same way the more accurate knowledge of vital phenomena will establish the conviction that a number of physiological properties depend on chemical composition; and physiology, when it shall have taken up animal chemistry as a part of itself, will possess the means of investigating this relation of dependence. It will then be able to find a juster expression for physiological phenomena."

But elsewhere he makes it plain that by such expressions as these he did not by any means mean to imply that in chemistry and physics we have the whole foundation of physiology. Thus later on he writes:—

"It is certain that a number of effects observed in the living body are determined by chemico-physical causes, but it is going much too far to conclude from this that all the forces which act in the organism are identical with those which govern dead matter . . . . Those who adopt such an opinion have lost sight of the first and simplest rule of physico-chemical inquiry, which directs us to prove that an effect which we ascribe

* Third edition.

to a cause is really due to that cause, and no other." And he closes this chapter, after showing how little we know even of the known forces and their relations, by saying that this imperfect state of knowledge of the essence and effects of known natural forces "explains how it comes to pass that at this moment we are unable to solve the question in reference to the existence of a peculiar force or influence acting in the living organism, by the method of exclusion or elimination."

Bischoff, who was Liebig's intimate friend and correspondent, has shown us by what he has said on this subject that Liebig himself did not deny that there may be a distinct vital force which determines the production of organised structures, and works in them with the chemical and physical forces. Vital force for Liebig was most nearly like chemical force and related to organised structures, somewhat as chemical force is related to chemical compounds.

The tendency of physiologists and pathologists, for a long period before Liebig joined in their work, had been to attempt to investigate the most complex phenomena of life before they were acquainted with the simplest. In his efforts to apply organic chemistry to physiology Liebig threw himself with characteristic enthusiasm into the company of those who were beginning to realise the inevitable sterility of such a method. It was, as has been said, his object and theirs to introduce into physiology the method which in a few years had revolutionised some branches of chemistry—the method, that is to say, of proceeding from the simple to the complex. It seemed to him that chemistry and physiology had reached such a stage that a fusion at the boundaries of these

two sciences might be looked forward to as one of the most striking results of scientific progress that would be attained in the future. Physiology, he thought, could no longer dispense with chemistry in the study of vital phenomena, and chemistry was prepared—largely by Liebig's own work, it must be added—to enter into the service of physiology.

These, then, were his objects as a student of physiology—to make the methods of physics and chemistry the methods of this subject also, and to bring about a union of chemistry and physiology that should result in a new and a greater physiology, which should embrace these two great branches of knowledge which had been too long separated.

The measure of his success can be best expressed by quoting the following extract from the writings of Bischoff, who knew Liebig, and who knew physiology both before Liebig set his hand to his self-imposed task and after his labours were over. Nearly at the end of his address on the influence of Liebig on the development of physiology, he said: "However great and immortal may be the services of Aristotle, Galen, Vesalius, Harvey, Linnæus, Haller, Cuvier, Charles Bell, J. Müller, and others to physiology; however greatly they and others may have enriched our knowledge in the province of organic nature, I can find no proof in the history of science that any single one of these has so influenced and altered the views and methods of physiology, and also consequently of medicine, by facts and methods as Liebig. I regard Liebig as the one who has brought organic natural science into the pathway of exact investigation."

And now let us return for a moment to Liebig's great generalisations on the relations of plants and

animals to each other and to mother earth. It will not be necessary to dwell for long on this topic, for most readers by this time will have discovered for themselves the main features of the wonderful cycle of changes which Liebig's mind first made plain to us. They have learnt from him how plants absorb from the air and from the earth simple inorganic substances —carbonic acid gas, water, and ammonia, with a few mineral salts; how by the help of sunlight these are so altered that the oxygen of the carbonic acid gas is returned in the elemental state to the air, whilst the rest are retained, and go to form first the various parts of vegetables, afterwards the blood, the flesh, the bones, and the nerves of animals; and how, either in the bodies of the herb-eating animals, or in those of the flesh-eaters, the carbon in the course of time again enters into combination with oxygen, which the animals absorb from the air by their lungs, and is returned to the air, except so far as concerns that portion which is retained in their tissues by young growing animals. How in the same way the nitrogen of ammonia, or of nitrates, and the mineral matters are also thrown off, at a rate approximately equal to that of their consumption, and returned, the salts to the earth, the ammonia to the air or earth; and how finally, on the death of the animal, every trace of its components return by the processes of decay to their original condition of lifeless inorganic matter, ready to nourish new generations of plants and animals, provided only that man does not—as, alas, he too often does—disturb the balance of nature by casting into the sea that which should be used for the enrichment of the land.

Looking at the matter from another point of view,

we have seen, with Liebig's aid, how the plants are, as he expressed it, "the accumulators of force;" how these, utilising some of the energy, as we now call it, which reaches them from the sun, reduce carbonic acid gas, so that its carbon enters presently the bodies of animals in such forms that it can afterwards liberate, on its re-union with the oxygen, supplies of energy which are available for doing work in its various forms, or for maintaining the necessary temperature of the organism. In short, if we have not learned from Liebig quite all that science can teach us to-day on this subject, it is no fault of Liebig's, but due to the fact that the "Physicists" were not quite ready for the chemists when Liebig attacked this question.

CHAPTER VIII.

EDUCATIONAL AND OTHER WORK.

Influence on the Methods of Education—Giessen Laboratory—Liebig as a Teacher—Founding of other Chemical Schools—His Attempts to carry results of his Studies to those outside the Universities—Interest in Technical Education—What is Technical Education—Supposed "Spontaneous Combustibility" of Human Bodies—Literary Work.

It might reasonably be supposed by anyone who had become acquainted for the first time with so much of Liebig's work in science as it has been possible to allude to in the foregoing pages, that surely this must be the sum of it. That he must have spent his days and nights in his laboratory and his study; that as a teacher he must have been content to deliver his university lectures, and have devoted all the time not occupied by them to meditating, experimenting, and writing. But this was by no means the case. Liebig made a great departure in the teaching of chemistry. And we may even venture to call him the earliest of "extension teachers," since he had the happy idea of conveying to the general public some notion of the progress of his science in his university and elsewhere, by means of a series of letters which he addressed to the *Augsburger Allgemeine Zeitung*. In these letters he gave, from time to time, the results of such of his inquiries as he could render intelligible to general readers.

A very prince of extension teachers, surely! Can

it be doubted that these wonderful letters—"Liebig's Familiar Letters on Chemistry"—had much to do with the present intelligent attitude of the German "practical man" to science, which has contrasted so strangely with that of his average English brother for many years past, much to the material disadvantage of the latter, it is to be feared.

We have already learnt that on his return to Germany from Paris Liebig obtained, through the interest of Humboldt, an appointment first as extra-ordinary, and soon afterwards as ordinary, professor of chemistry in the little German University of Giessen. Here he stayed for twenty-eight years. Speaking of his life there, he said afterwards, "It was as if Providence had led me to the little university. At a larger university, or in a larger town, my energies would have been divided and dissipated, and it would have been much more difficult, and perhaps impossible, to reach the goal at which I aimed; but at Giessen everything was concentrated in work, and in this I took passionate pleasure."

A few years before Liebig went to Giessen, it was, as he has told us, "a very wretched time for chemistry in Germany." Only the pharmacists had preserved any remains of practical instruction, and in order to get any real training it was necessary for Germans to go to Paris or to Stockholm. Even in those places the student found no public laboratories, but had to rely on such interest as he could command to gain admission to the workroom of some chemist of distinction, which, after all, was sometimes only a garret or a cellar. Liebig, as we have seen, became the pupil and friend of Gay-Lussac, and in his laboratory formed the idea of effecting a reform

at home. Soon after receiving his appointment, he took his first step by opening his first—or, rather, the first—public laboratory for the teaching of practical chemistry in Germany. Pupils soon began to stream in, so that the teaching of analysis and other parts of practical chemistry had to be organised on a systematic plan. This necessity led to the introduction of a system of teaching elementary and advanced practical chemistry on which all his successors have founded their methods.

Actual teaching in the ordinary sense in practical work was only given in the Giessen laboratory to beginners; this work was put into the hands of skilful assistants. Liebig himself took charge of the special pupils; their progress largely depended on themselves. Their master gave the task, and supervised the doing of it. There was no actual instruction. Liebig received every morning from each pupil a report upon what he had done on the previous day, as well as his views upon the work. He approved or criticised this, but every student was obliged to follow his own course. Owing to constant intercourse each student participated in the work of all the others; every one was a learner, every one was a teacher. Twice a week Liebig gave a review of the most important questions of the day—a report of his own work, combined with that of the students and with the researches of other chemists.

Work went on from daybreak till nightfall. There was no amusement and no dissipation to be had in Giessen, and the only complaint, which was a frequent one, was that of Aubel (the laboratory man), who could not get the workers to depart in the evening, when he wanted to clean the laboratory.

In his practical teaching Liebig laid great stress

on the producing of chemical preparations—on the students preparing, that is to say, pure substances in good quantity from crude materials. The importance of this was, even in Liebig's time, often overlooked, and it was, he tells us, more common to find a man who could make a good analysis than to find one who could produce a pure preparation in the most judicious way. There is no better way of making oneself acquainted with the properties of a substance than by first producing it from the raw material, then converting it into its compounds, and so becoming acquainted with them. By the study of ordinary analysis one does not learn how to use the important methods of crystallisation, fractional distillation, nor acquire any considerable experience in the proper use of solvents. In short, one does not, as Liebig said, become a chemist.

Unfortunately, owing to the readiness with which analysis lends itself to competitive examinations, the importance of this side of experimental chemistry has for many years been greatly exaggerated by many English teachers. There are signs, however, that sounder views are now reasserting themselves in the minds of teachers.

It was during the early years at Giessen that Liebig busied himself with the task of improving the methods of organic analysis: and in a few years, when this work was completed, the Giessen laboratory must have presented a busy scene. Talented young men were now streaming in, not only from Germany, but from all parts of Europe and from the United States of America; and Liebig had working under his supervision a band of twenty or more skilful and indefatigable young chemists, by whom thousands

of experiments and analyses were carried out every year. From their results the master gained an abundance of the facts and materials out of which he developed the wonderful conceptions with which he enriched chemistry, physiology, and agriculture.

Liebig's system of teaching the methods of research to advanced students gives a "character" to scientific education in Germany to this day. Students, when they come back from Germany, tell us that in German universities the lectures and elementary practical instruction are, if anything, inferior, certainly not superior, to the teaching in our own schools and universities, and that the well-taught Englishman when he goes to a German university is certainly not at a disadvantage in this respect. What he finds there, and what it is difficult to find at home, in spite of the great and valuable contributions of our countrymen to science, is an opportunity for soaking himself with the spirit and methods of research by joining a numerous band of young and devoted workers, with whom he will be in constant intercourse from eight in the morning to six at night, hearing and seeing nothing for a year or two but original experiments all conducted under the guidance of a master mind.

All this, the preliminary courses, on which we at home may pride ourselves, the great centres of organised scientific investigation which are the glory of Germany, we owe to the early labours of Liebig in his little laboratory at Giessen! Through his work the Giessen University became one of the forces of the world.

When we remember that a very few years before Liebig went to Giessen he could not find in

all Germany any one who could teach him chemistry, and that, as a lad, he had followed a professor from Bonn to Erlangen, in order that the professor might carry out a promise to analyse some minerals with him, only to find that, unfortunately, his guide did not himself know how to do it, we can hardly sufficiently admire the energy and insight which, in a few short years, enabled Liebig to build up a complete system of public instruction, which in its main features still seems to us to come very near to being a council of perfection.

In this, the first—one might almost say, the ideal—school of chemistry, Liebig gathered round him the *élite* of the ambitious and able young students in this science from the whole civilised world, so that an immense number of the chemists of to-day owe their education, either directly or indirectly, to him. From England students flocked to him, so that English chemistry especially came under his influence, the more so that Hofmann, his brilliant pupil, afterwards directed the first chemical laboratory opened in England for giving public instruction in the science.

This is not the place for introducing a long list of the names of Liebig's eminent pupils. Such a list would only be interesting to a limited number of readers. It will be sufficient, therefore, to say that Liebig's English pupils have shown his influence not only by their work in pure chemistry, as teachers and as investigators, but that they will be found equally eminent among the ranks of those who have devoted themselves to the applications of chemistry in the various departments of industry.

Liebig as a lecturer was not, it seems, exactly eloquent, but his personality was magnetic, and he

could say what he wanted. He had a greater gift than eloquence. He could stimulate the reflective powers of his pupils and make them think. This was the secret of his success and theirs.

Hofmann says of him that "whoever has had the good fortune of attending his lectures, will not easily forget the deep impression of his peculiar style of eloquence. Liebig," he says, "was not exactly a fluent speaker, but there was an earnestness, an enthusiasm in all he said, which irresistibly carried away the hearer. Nor was it so much the actual knowledge he imparted which produced this effect as the wonderful manner in which he called forth the reflective powers of even the least gifted of his pupils. And what a boon it was, after having been stifled by an oppressive load of facts, to drink the pure breath of science, such as flowed from Liebig's lips. What a delight, after having perhaps received from others a sackful of dry leaves, suddenly, in Liebig's lectures, to see the living, growing tree."

It was not, however, really in lecturing that Liebig excelled, but in the laboratory. Here he was followed not only because he was admired, but because he was loved still more. His pupils felt a pleasure in submitting themselves to his fascinating control. And Hofmann tells us that if anything could exceed their wonder at the amount of work he did himself with his own hands, it was the wonder they felt at the mountain of toil which he got them to go through themselves; that, if anything exceeded their pride in his friendship and approval, it was their delight to know that they helped him, and that whilst they were receiving his lessons they were also performing his work. The aid that he obtained in

this way was doubtless considerable, but he never failed to give full credit for such aid, and, indeed, so far was he from underrating it, that in his generosity he often gave a pupil the whole credit for a success obtained by experiments suggested by himself and based upon his own previous trials and discoveries.

In his autobiographical fragment, from which I have made so many extracts, Liebig, when speaking of the wonderful chapter of accidents by which he made the acquaintance of Gay-Lussac, says that "since he found favour as a boy in the sight of Thénard, Humboldt, Dulong, and Gay-Lussac, it had frequently been his experience that marked talent awakens in all men," he believes without exception, "an irrepressible desire to bring about its development." Whether he goes too far in saying this or not, there can be no doubt that in Liebig's own character this instinct existed in a most marked degree.

The founding of Liebig's School of Chemistry at Giessen formed an epoch in the history both of science and of education. Almost from the day when this school was inaugurated, chemistry and the branches of science connected with chemistry have advanced by leaps and bounds. It is impossible not to find one of the most potent causes of this advance in the work that was done at Giessen by Liebig. The great success of this laboratory quickly led to the building of others, and soon every university at least possessed such a laboratory. At first they were modest places enough, many of them, till Hofmann on his recall from England to Germany was provided with something very like a palace at Bonn. It is customary, in Germany, to provide the professor with a residence in the laboratory, and so fine were the apart-

ments provided at Bonn that the King of Prussia, when he opened the building, is said to have remarked: "I should like to live here myself." Soon afterwards a second palatial building, also for Hofmann, was built at Berlin, and thus an era of palatial laboratories was inaugurated. Other countries, including England, followed suit more or less quickly, and now, as we all know, not only the universities, but all our chief towns, are provided, some with one, and some with more than one, laboratory, which, though not always—nor, indeed, often—a palace, is efficient and fully equipped for teaching and frequently for chemical research. Indeed, few towns of even the second and third rank are now quite without some provision for the practical study of the subject, whilst our leading schools have also, in the great majority of cases, provided themselves with similar equipment. Much doubtless still remains to be done, but the progress made is already very great, and it may all be traced to the inspiration of Liebig.

And what is the result? Will the work done in these lecture-rooms and laboratories be worth their cost? In seeking to answer this question, we must not forget that the value of these places of public instruction in chemistry does not consist simply in the fact that they provide a certain number of workshops, more or less magnificent, more or less well provided, for the use of those who desire to study pure chemistry. They provide also practical instruction for that much larger and equally important class who purpose to devote their lives to the service of their fellows in medicine, in the various and ever-growing chemical industries, and in a variety of other directions too numerous to mention. And, again, we

must remember that chemical laboratories did not for long stand alone; presently other such work-places, physical laboratories, biological laboratories, engineering laboratories, rose beside them, ready to do for physics, biology, and engineering what the chemical laboratories were doing for chemistry. The result, neglecting altogether the immediate material gains, which though difficult to calculate must be enormous, is briefly this—that civilised mankind is rapidly learning the importance of facts and the value of the experimental method by which facts are gained, and is realising better than for many centuries the dignity and the intellectual value of intelligently-directed handwork.

So long as the recognised method of solving every problem was to sit down and think about it, it was not unnatural that those who worked with their hands should be looked upon as necessarily inferior to the head-workers. But the influences developed during the last fifty years are steadily changing the whole aspect of affairs, and it cannot but be that before long the intelligent hand-worker in every department will receive a fuller measure of esteem than has ever before, as far as we know, been awarded to him.

One of the most beautiful and fascinating experiments of the chemist is to prepare a very clear, strong, hot solution of a salt more soluble in hot than in cold water, and, after protecting this from the approach of dust, to let it stand till cool. If the experiment be perfectly made, such a solution will remain a solution—it may be indefinitely, at any rate for a long while; no crystals of the solid will make their appearance, though there is far more of it in the liquid than the water present can normally retain in

the liquid state. But if the most tiny, invisible fragment of the solid salt be allowed to touch the solution at any point, a change will at once set in, crystals will appear at that point, will quickly become visible, and will spread with ever-increasing rapidity, till in a few minutes or—maybe, in a few moments—what was before fluid will become a solid mass, as if by magic.

It was somewhat in this sense that the founding of Liebig's laboratory at Giessen was the cause of the movement which is now rapidly introducing what is sometimes called the scientific method in so many spheres of human activity. All the necessary elements for the change existed before Liebig went to Giessen. His fruitful idea acted like the crystal. It provided the necessary nucleus.

I have already spoken of Liebig as a very early exponent of the idea which is at the bottom of the "University Extension Movement." He endeavoured to bring the learning accumulated in the university to the people at large in two ways—first, by his celebrated letters on chemistry; secondly, by bringing to the university selected persons from the provinces to attend his courses of lectures, in order that on returning they might carry a little knowledge and some interest back with them into the provinces, and, maybe, sow the seeds which should produce greater things afterwards. This he made possible by inducing the Government, after his call to Munich in 1852, to grant stipends to a number of schoolmasters from different districts to enable them to defray the expenses of the visit to the city. He hoped in this way to point out the road to mental culture to those who had the task of educating the country populations.

Speaking of the results of this experiment, in 1861, he said in the course of an academical address: "It must be confessed that even among those who have not had the chance of attending our gymnasia, or our modern and technical schools, the opinion is rooted that a little more knowledge is of use even to the most common hand-worker; that some knowledge of botany to the gardener, some knowledge of chemistry to the soap-boiler, the tanner, or the dyer, is essential to the carrying on of their businesses; that a gardener is not a worse gardener for knowing a little of plant life; that a baker, because he knows what bread, meal, and salt are; or a soap-maker, because he truly knows the nature of fat, ashes, and lye, what are their good qualities, and how they may be recognised by tests—that these workmen will not be any the worse makers of bread or soap because they know this. Even the most simple citizen thinks it a gain if his neighbour, who may be a magistrate, has some knowledge of the principles on which the rules of health are established." Then, after discussing some defects in the German agricultural colleges as they then were, he went on to say: "The agriculturist and technologist must learn that he lives in a cruel time, when existence becomes more difficult every day for the incompetent and weak, and will soon perhaps be impossible." His remarks on this occasion are said to have given great offence. Fortunately for Germany, however, the lesson was by no means altogether thrown away, in spite of the opposition it aroused.

As has already been explained, Liebig's famous "Familiar Letters on Chemistry" were originally a series of letters published in an important South

German newspaper. They were afterwards collected, and on the occasion of preparing a third edition in 1851, Liebig took the opportunity of adding a number of letters on the origin and development of chemistry.

The first letter in this edition is especially interesting; in it Liebig lays down, with the greatest clearness, the practical relations of the sciences to one another, and that of the sciences to the arts with which they are respectively connected, and indicates the manner in which each of these must approach another for help and guidance; he shows the conditions which must be fulfilled, for example, before chemistry can help to solve the problems of physiology or contribute to the development of a chemical industry.

In many respects chemistry is, as he here points out, like mathematics. The latter teaches us in its application how to measure land, to erect buildings, to raise weights. In short, it is an instrument which confers the most obvious advantages on those who know how to employ it. On the other hand, mathematics, by means of a language of its own, teaches us how to draw logical conclusions according to definite rules, and to express them in a simple manner. We all know that mechanicians, physicists, and astronomers use mathematics; that it is an indispensable instrument for the ends they strive to reach. In order to make use of mathematics, they must become so expert in the subject that its management almost becomes a mechanical habit. But notwithstanding the value of this instrument, it is not the mere instrument which does the work. Besides having the necessary familiarity with the processes of mathematics, the mechanician or astronomer must be able to propound questions

to himself, and to test the truth of the answers given by mathematics. It is only if he has these further powers that he is qualified to undertake to investigate nature. Mathematics, the instrument, can only help astronomy, physics, and mechanics when it is in the hands of some one who can, so to speak, propound problems for it to be used upon. Besides a mastery of mathematics, the astronomer, the physicist, and the mechanician must possess acute observing power and imagination. These two last afford, so to say, the motive power. Of course the mathematics are equally essential, but they are not a cause, they are an instrument; mathematical work, as we know, may actually be to some extent done by machines, and we must not confuse the instrument with the cause in this case, just as we must not ascribe to a steam-engine results which have really been brought about by the human intellect.

And so it is with chemistry and the experimental sciences. These also—putting aside their value as a mental training, and in other ways which do not concern us at present—these also are instruments; and discoveries in chemistry, in physics, and so on, like discoveries in mathematics, are simply steps towards the perfecting of the instrument. Chemistry and the other sciences, like mathematics, each of them speaks a language of its own; and if we would apply any one of them to the service of man in this or that department, we must be thoroughly acquainted with that language. But here again, as in mathematics, these physical sciences do not make discoveries for us. If we apply chemistry to a chemical problem set out in the right way, chemistry will find us the answer to that problem, provided, of course, that we

do not go beyond the powers of the instrument for the time being. But the question must be clearly and definitely put. If the ideas of the inquirer are not clear as to the problem to be solved, chemistry can give him no help. The inquirer, in short, must understand the instrument as a whole; it is not enough that he shall have seen a wheel here and a wheel there; he must also have sagacity and observing power; he must be able to propound definite questions, and understand how to test the truth of the answers obtained by yet other questions when that is necessary.

"Can men," asked Liebig, "who do not apprehend the nature of scientific investigation in a philosophical spirit, and who cannot interpret the language of phenomena—can such men be expected to derive the least advantage from the discoveries of chemistry or physiology; and can they be deemed capable of making the most insignificant application of those discoveries to practical purposes?"

These were the considerations which led Liebig to take great interest in technical education; which led him to praise the apothecary, who, after learning the elements of his art in the shop, resorted to the university for instruction in the science on which it rests; and to blame those physiologists and agriculturists who had objected that chemistry could and did give them no answer to their questions, when they had not learned the language of the instrument, and hence were unable to put the questions to which they desired to have answers.

Has not Liebig, by his teachings on this point, answered beforehand very plainly a question which we often hear nowadays—What is technical education?

Does it mean the teaching of the arts, or does it mean teaching the sciences on which those arts are based? And again, as regards these sciences, may a technical student learn only just so much of them as directly applies to his art; or must he, like other students, aim at the best all-round knowledge of them that he can attain?

Of course it is obvious that if a youth is to become a baker he must learn baking in the bake-house; that if he would be an industrial chemist he must sooner or later enter a chemical works; that he who would be a farmer must spend years upon a farm.

For the rest, what Liebig wrote in 1851 still conveys the whole truth in 1894—

"Hitherto," said Liebig, "scarcely any demand has been made upon the science of chemistry* by arts, manufactures, or physiology, which has not been responded to. Every question clearly and definitely put has been satisfactorily answered. Only when the inquirer had no precise idea of the problem to be solved has he remained unsatisfied."

Clear and precise problems can only be put in chemistry and the other experimental sciences, as in mathematics, by those who are well acquainted with their method and language, and who have learned how to set such problems.

It follows that he who, "in these cruel times," would take advantage of the resources of science in his art, must study the sciences on which it depends, so far as he can, in the same way as other students. That is to say, he must learn the "go" of each instru-

* He was then speaking for chemistry only, but his words are not less true of other sciences.

ment as a whole; he must not be satisfied with a peep at this part and a peep at that part. Above all, his scientific training must cultivate his powers of observation and his powers of putting questions to nature to the utmost possible extent. The needs of the technical student, in short, do not differ essentially at the early stages from the needs of those who will afterwards devote themselves to pure science.

Liebig on several occasions did good service by helping to dispel popular errors. This was notably the case in regard to the "spontaneous combustion" question.

In one of those delightful discourses, the "Familiar Letters," Liebig treated of the at one time much discussed question, Is it possible for a human body to ignite spontaneously? The notion that a living human body is capable of spontaneously catching fire appears to be comparatively modern. The earliest recorded example which Liebig could discover was said to have occurred in 1725. A woman of Rheims was found half a yard from a fireplace burnt to death; very little was left of her. Her husband had an attractive servant girl, and murder was suspected. But at the trial learned experts declared spontaneous combustion to be possible, and the husband was pronounced innocent.

This idea, therefore, arose before Lavoisier had explained the nature of combustion, and when men were not in a position to understand such phenomena. It persisted, however, long after Lavoisier had explained the chemistry of fire. And Liebig himself on one occasion was called as a witness, when this delusion had been set up in defence of a man-servant who was accused of the murder of his mistress, the Countess

of Görlitz, in Darmstadt. From 1725 till Liebig investigated the subject—that is, during rather more than a century—some forty-eight cases of supposed spontaneous combustion had been recorded; and so firmly fixed was the belief in the possibility of the phenomenon, that even after Liebig had demonstrated the complete absence of any real evidence in support of its truth, and after it had been finally abandoned by men of science, and had ceased to be admitted in the law courts, it retained a sufficient hold to be still made use of in fiction; and Charles Dickens, indeed, published a defence of one form of the doctrine in the preface to an edition of "Bleak House."

It need scarcely be said that Liebig was not the first to feel doubtful as to the possibility of this extraordinary phenomenon. Others before him had rejected the popular idea as improbable and incredible; but till Liebig's arguments at length exploded the belief it remained floating in the heads of some doctors and lawyers, and it was accepted by them as at least an open question.

Liebig found, on investigating the history of the recorded cases, that on most occasions the victims were intoxicated; that the supposed spontaneous combustion occurred usually where the rooms are heated by open fires or pans of glowing charcoal, and were rare in countries like Germany, where closed stoves are employed; that they occurred in winter; that no trustworthy witness had ever yet seen the phenomenon occur; that the physicians who collected the data had never really known the history of the cases they attempted to explain; and that the amount of combustible matter on the spot was never ascertained. When he came to look into one of

certain cases which were specially reserved, as seeming to be established by Dr. Franck, he found, omitting many corroborative circumstances, that, in this case, before the combustion took place, there was a lamp full of oil in the room, and afterwards the lamp was empty and the wick burnt up; that the body was only burnt where the clothes were also burnt; finally, that while the clothes were in many parts burnt to ashes, the skin was only detached, hanging in shreds; from which it would seem to follow, if spontaneous combustion had occurred, that the kindling of the clothes was caused by the skin, which yet itself did not burn.

A great many writers in Liebig's time, perhaps the majority, did not, however, suppose that in these forty-five or forty-eight cases the victims had really ignited spontaneously—had caught fire, that is to say—without the aid of an external flame. The most commonly-accepted view was rather that, though the healthy human body is very difficult to burn, the flesh and skin may become much more combustible through diseased conditions produced, for example, by the excessive use of strong spirits; that they may become not merely like a block of wood, which easily goes out when kindled, but more like, let us say, tinder, which, when once ignited, continues to burn. The supporters of the theory said that the fact of spontaneous combustion in this sense was not refuted by all that science then knew; that circumstances and facts were quite concordant with this view; and they asked whether, considering how many phenomena there are which science has not yet explained, it was fair or decent to reject the testimony of so many upright men who have avowed their belief in spontaneous combustion, and to class

them with liars or blockheads because we do not agree with them.

Liebig pointed out, in reply to this, that nobody denied the facts stated—viz. that a number of people had been burnt; what was denied was the hypothesis by which the facts were accounted for. It was plain that every kind of assertion could be supported on such grounds as these. And, moreover, that the opinion that a man can burn of himself was obviously not founded on a knowledge of the exact circumstances of death, but, on the contrary, was based upon ignorance of these circumstances.

A man or woman is found in a room burned to death. The experts, after an inquiry, cannot discover how the fire originated, or how it was propagated in the body; therefore, because at intervals during a century or more in the past similar unaccountable deaths from fire have occurred, and have been ascribed to spontaneous combustion, we are asked to ascribe these deaths also to the same cause. And this in spite of the fact that our predecessors appear to have been quite as ignorant as ourselves of the facts of the respective cases, were not even present at the time of the occurrences, and that their explanation is not only without any independent support of a definite kind, but also contradicts all that we know about the combustibility of animal matter.

Before it can be admitted that a given death by fire was due to the body having acquired a morbid state, in which it becomes as combustible as straw, we must, said Liebig, "not only prove that it is possible for a piece of flesh to become thus combustible, but we must prove that such combustion, when it occurred, has proceeded from the flesh

outwards . . . it must be shown that a morbid state, such as is assumed, actually exists; and further, that the persons who were burned were in that morbid condition."

"Now, nothing of the kind has ever been done; no doctor has ever observed a condition of the human body in which it was readily combustible; and no one knows any signs by which we could recognise such a condition."

In concluding this letter Liebig made a valuable contribution to medical jurisprudence, which, though it related only to the question of spontaneous combustion, upheld a really important principle.

The physician, he said, "who is called on for a judgment in such cases can only say, if he act according to duty and conscience, in what state the body was found; whether the injury from burning took place before or after death; whether death was caused by fire alone, or before the action of the fire by other causes, such as wounds, strangulation, a blow on the head, etc. In no case is it permitted him to explain anything he has not seen by cases which he has also not seen, or by a theory which he cannot understand." Those who disregard these limitations protect criminals, and delay or prevent the administering of justice.

The comparative ease with which Liebig overthrew this popular fallacy throws an interesting sidelight on the progress of civilisation in Europe. It had now become not only safe to attack a popular error, but one might even hope to destroy it. This was not so a little earlier. We are told that when Kepler the astronomer went to Tübingen, in the sixteenth century, to save his mother from the stake, he

succeeded not by proving that there are no such people as witches, but only by showing that she possessed none of the characteristic signs essential to a witch.

This was not the only occasion on which Liebig set himself to correct popular misconceptions. Thus when, after the discovery of electro-magnetism, his less instructed countrymen were pleasing themselves with the idea that electricity would replace steam for putting machinery in motion, and that, because zinc is used for generating electricity, and is plentiful in Germany, soon Germany would become the chief seat of the manufactures in place of England, Liebig was quick to point out to them that such expectations, though attractive to the human mind, are quite fallacious, unless they are the outcome of exact comparisons and calculations. "Out of nothing," said Liebig, "no kind of force can arise." From a pound of coal we get about six times as much force as from a pound of zinc when we burn them, and hence, even if it were true that the latter produces four times as much force in a galvanic pile as it does when we burn it, the coal would still be the more economical substance in practice. Besides, coal is wanted to reduce zinc from its ores, and it is probable that, if the coal required for smelting the zinc were burned under a steam-engine, we should obtain much more force than all that the zinc reduced by means of it could yield us.

These are a few illustrations drawn from this remarkable book, into which Liebig put so much of his very best work and thought in a form in which they could help every intelligent person who would read them. They are, as Liebig meant them to be, a great deal more than mere popular expositions. In these letters science is freed from technicalities, but Liebig

did not allow himself to attenuate the science in getting rid of the technicalities.

It has been necessary in the preceding pages to refer to several of Liebig's most important books, and it has been mentioned that the mere list of his memoirs to scientific journals occupies twenty-three columns of the Royal Society's catalogue, and includes 318 separate papers; but this does not give anything like a sufficient idea of his immense contributions to, and influence on, scientific literature. In 1832 he founded one of the most celebrated of scientific periodicals, known generally as the *Annalen*, in the editing of which he had for a long while the assistance of his friend Friedrich Wöhler and of Hermann Kopp, the historian of chemistry. It may be imagined what this alone meant when it is said that no less than 165 volumes of the book had appeared at the time of Liebig's death in 1873. In the *Annalen* are to be found all the researches carried out by Liebig and by his pupils at Giessen, from the time of their first publication. Jointly with Wöhler again, and with the co-operation of Poggendorff, he was author of a dictionary of pure and applied chemistry, which was for long a most valuable resource, and on which a later dictionary by Fehling was founded. Besides this he gave chemists the benefit of his unexampled mastery of organic chemistry in his handbook on that subject, the origin of which did as much credit to his heart as its execution did to his head. It appears that shortly after the death of an early friend, P. L. Geiger, a new edition of his once celebrated book on pharmacy—"Geiger's Handbook of Pharmacy"—was called for, and that Liebig, with characteristic generosity, undertook to revise the

chemical part of it in the interest of his dead friend's widow. Soon, however, revision was found to be impossible; so great were the strides which chemistry had made in the interval since Geiger had published his last volume, that Liebig had to give up the idea of revising the work, and proceeded to re-write it, with the result that the new "Handbook of Organic Chemistry" soon appeared. This book had a wonderful success; what had previously been a maze of incoherent information became in this volume for the first time an articulated science, it was soon translated into French and appeared also in England as a part of Turner's "Chemistry" in the later editions of that work.

On the death of Berzelius, he was called by the unanimous voice of chemistry to continue the Annual Reports on the progress of chemical science which Berzelius had for many years drawn up. When it is mentioned that for the compiling of these reports the work of a small army of investigators, published in perhaps a hundred journals, and in several languages, had to be read and sifted, it will be understood what a labour this work must have involved, and how great a service Liebig did his fellow chemists by undertaking it. And even when all this is told, there remain his various discourses on subjects of popular interest, his reports (1) On the State of Chemistry in Austria, and (2) On the State of Chemistry in Prussia, which exercised a most marked influence on the progress of scientific education in Germany; and some minor books and pamphlets, enough by themselves to have made a great reputation.

CHAPTER IX.

CHARACTER AND LATER YEARS.

Dominant Characteristics of Liebig—Address to Bavarian Academy after Franco-Prussian War—Relations with English Men of Science—Letter to Faraday—A Testimonial from England—Liebig and his pupils—Munich and his later years.

The dominant characteristics of Liebig were his intense desire for truth, his unselfishness, the complete absence from his mind of any tincture of the partisan, and his unfailing vivacity.

His scientific disputes were, from the novelty of many of his ideas, not, unnaturally, rather numerous. In scientific discussions he too often forgot the man in his desire to rend and destroy the error. In his ardour he doubtless sometimes forgot that his opponents, like himself, were animated, in most cases at least, by a desire to promote the discovery of truth and the overthrow of error. But there was so much to admire in Liebig, so much to love when you knew him—and he was so ready to admire what he saw to be true even in the work of an opponent—that when the battle was once fought out reconciliation was in most cases easy; and we find, for example, that, in spite of the vivacity of his encounters with Dumas, these two great men were more than once to be found working together. And again, that in spite of the discussions with Laurent and Gerhardt, in which Liebig took an active part, and in which the two former chemists had to suffer treatment which

has since been described as "quite painful to think of," Gerhardt described his meeting with Liebig at Munich some time afterwards to Hofmann with "glowing delight."

Nor must we forget, when reading Liebig's most lively passages, that they were not written yesterday. Neither in pugilistic encounters, nor in scientific disputes, had it then become the custom to fight with the gloves on; and, besides, his antagonists were by no means unable or unwilling to give very hard knocks themselves when an opportunity arose.

Liebig was one of the first eminent Germans after the Franco-Prussian War to hold out the olive branch. At a moment when the irritation on both sides was still keen, he attempted, in an address to the Bavarian Academy, to soothe the feelings of the moment by an eloquent appeal to the traditions of the glorious past.

"This," said Liebig, "is, perhaps, a fit opportunity for declaring, on the part of our Academy, that a hatred of race between the German and Latin nations does not exist.

"We look on the heavy affliction which in former times the French nation has caused to Germany as on an illness, the pains of which are utterly forgotten with the return of health.

"The peculiar nature of the German, his knowledge of languages, his appreciation of other nationalities, compel him to do justice to foreigners, so much so as occasionally to become unjust to himself; and thus we cannot possibly underrate the debt of gratitude we owe to the great philosophers, mathematicians, and natural inquirers of France, who, in so many departments, have been our masters and exemplars. . . . .

"Warm sympathy for all that is noble and great, and disinterested hospitality, are among the finest features of the French character. It will be on the neutral ground of science that the best minds of the two nations must meet in endeavouring to reach the high goal common to both, that these sentiments will be again kindled into life and activity; and thus the feeling of fraternity in science, which can never be entirely extinguished, will gradually contribute to mitigate the bitterness with which the deeply wounded national pride of the French is filled by the consequences of the war they have forced upon us."

The last few words might have been different, but they were said in 1871. The rest formed a characteristic tribute to the nation who had fostered and helped to inspire Liebig himself half a century before.

Liebig's relations with the English men of science were of the most cordial character. After his first visit to England, in 1837, he wrote on his return that he had seen astonishing things. At that time Thomas Graham alone, among the chemists, made a very strong impression on him, and he noted with regret that the opportunities for learning chemistry were then bad. English students went to Germany in large numbers to become his pupils. Two of the most important of his works were presented to the British Association, as we have seen, and most of his other books were translated into our language; consequently his influence on the progress of chemistry in this country has been very great. In the course of his repeated visits he became acquainted with Faraday, for whom he conceived feelings of the most

profound admiration, respect, and friendship. The following letter from Liebig to Faraday is interesting alike for the light it throws on the relations between these two great men, and for the delightful picture, mere sketch though it be, which it contains of Liebig's home life in Giessen :—

"GIESSEN, *December 19th*, 1844.

"DEAR FARADAY,—I intended to have written you long ago of my safe arrival, and that I had found my wife and children well. The opening of my winter course, and a mass of work which had accumulated during my absence, have hitherto prevented my fulfilling my intentions. Now, however, that I have a few days of rest during the Christmas holidays, I will not let the opportunity slip of wishing you, with my whole heart, a merry Christmas and a happy new year. Often do my thoughts wander back to the period which I spent in England; among the many pleasant hours of which the remembrance of those passed with you and your amiable wife is to me always the dearest and most agreeable. With the purest pleasure I bring to mind my walk with her in the Botanical Garden, at York, when I was afforded a glance of the richness of her mind: what a rare treasure you possess in her! The breakfast in the little house with Snow-Harris and Graham, and our being together at Bishopthorpe, are still fresh in my memory. I wish it were only my good fortune to see and talk with you oftener, and to exchange ideas with you.

"Nature has bestowed on you a wonderfully active mind, which takes a lively share in everything that relates to science. Many years ago your works imparted to me the highest regard for you, which has continually increased as I grew up in years and ripened in judgment; and now that I have had the pleasure of making your personal acquaintance, and seeing that in your character as a man you stand as high as you do in science, a feeling of the greatest affection and esteem has been added to my admiration. You may hence conceive how grateful I am for the proof of friendship which you have given me.

"I have every reason to be satisfied with my journey in Great Britain: rare proofs of recognition have indeed been given

me. What struck me most in England was the perception that only those works which have a practical tendency awake attention and command respect, while the purely scientific works, which possess far greater merit, are almost unknown. And yet the latter are the proper and true source from which the others flow. Practice alone can never lead to the discovery of a truth or a principle. In Germany it is quite the contrary. Here, in the eyes of scientific men, no value, or, at least, but a trifling one, is placed on the practical results. The enrichment of science is alone considered worthy of attention. I do not mean to say that this is better; for both nations the golden medium would certainly be a real good fortune.

"The meeting at York, which was very interesting to me from the acquaintance of so many celebrated men, did not satisfy me in a scientific point of view. It was properly a feast given to the geologists, the other sciences serving only to decorate the table. The direction, too, taken by the geologists appeared to me singular, for in most of them, even the greatest, I found only an empirical knowledge of stones and rocks, of some petrifacts and a few plants, but no science. Without a thorough knowledge of physics and chemistry, even without mineralogy, a man can be a great geologist in England.

"I saw a great value laid on the presence of petrefactions and plants in fossils, whilst they either do not know or consider at all the chemical elements of the fossils—those very elements which made them what they are.

"This letter has already grown too long, and truly I fear to weary your patience. I cannot, however, deny myself the pleasure of expressing a sincere wish to see you and your wife here in Giessen next summer. Did you know how quietly we live at our German universities you would certainly expect from your visit only benefit to your health. Except scientific pursuits we have no other excitements of the mind. We take walks in our beautiful green woods, and in the evening drink tea at the neighbouring old castles. This is our recreation. I beg of you, dear Faraday, to listen to my request. I pray your dear wife to assist me in trying to make you decide on this journey. My wife unites with me in begging this, it would give her the greatest pleasure to make the personal acquaintance of you and your lady.

"Farewell, dear Faraday, preserve to me your friendly favour, and believe me, with all sincerity, to be

"Yours very truly,

"DR. JUST. LIEBIG."

One cannot help seeing in this letter a charming simplicity of mind which reminds one of Faraday himself. Different as these two men were, in many respects, they resembled one another in this, and in a certain modesty, which was never out of sight in the case of Faraday, and showed itself in Liebig, too, upon occasion—as, for example, when, as a young man, he wrote to Wöhler, in 1830, regretting that they had never yet met, and expressed a fear that Wöhler would find out later his (Liebig's) real poverty of acquired knowledge—and which led him to say so little about the various scientific distinctions which were liberally showered upon him, even before he reached middle life, that frequently many of his intimate friends were unaware that he had received them. Nor was this silence due to want of appreciation. This is shown by the correspondence given below, which took place on the occasion of his accepting the call to Munich, when Liebig's English friends united, under the presidency of Graham, to recognise the occasion by presenting a testimonial to him:—

"LONDON, *July*, 1854.

"SIR,—Your retirement from the Chair of Chemistry in the University of Giessen has appeared to many in this country a fitting occasion for the public acknowledgment of your eminent scientific services. Accordingly a numerous body of your friends and admirers have united to present to you a Testimonial, commemorative of their profound and unalterable regard. In the list of subscribers hereto annexed, you will recognise, with those of your pupils and personal friends, the names of many other gentlemen eminent in science, in

social position, and in the practical arts of life, who were anxious to join in this just tribute to your merit.

"In presenting to you this Testimonial, the subscribers desire to express their sense of the benefits which your genius and labours have conferred upon mankind in adding to the world's stock of positive knowledge. These benefits are limited to no one people or time; but it is felt that Englishmen may, with propriety, take the lead upon this occasion, as the impulse which you have given to chemical science has been experienced especially in England. More students from this country than from any other land beyond the bounds of Germany, have worked in the laboratory of Giessen, and have derived incalculable benefit from the instruction there imparted, and from the noble example there presented to them of an elevated philosophical and scientific life. In England, also, have the applications which you have made of chemical science to the cultivation of the soil been peculiarly appreciated and adopted.

"Your discoveries in practical agriculture have enriched the land, and with you originated the method of scientific inquiry which is here pursued on an extended scale by numerous investigators, and which is rapidly changing the features of the most ancient and important of human arts.

"We earnestly hope that your life, which has been devoted to the highest aims to which man can aspire, may be prolonged to many years of happiness and honour.

"Signed on behalf of the subscribers,

"THOMAS GRAHAM.

"To Baron Liebig."

Liebig's reply :—

"MUNICH, *July 20th*, 1854.

"SIR,—The man of science generally knows of no other reward for the time he has devoted to the discovery of truth and to the investigation of the laws of Nature's powers, than the mental satisfaction which springs from the consciousness of having, to the best of his ability, contributed his part towards the advancement of human happiness and human welfare; for toils like his, attended as they are with so many difficulties and sacrifices, and with such mental effort and fatigue, cannot be priced in the market or sold—cannot be performed to order.

or turned into money. If he has been fortunate enough to have gained by his successes the acknowledgment and esteem of his contemporaries, he has obtained the highest object of his ambition.

"If I have laboured for the period of almost a human life in promoting the progress of chemistry, and in making its principles subservient and useful to other branches of knowledge, more especially to the industrial arts and to agriculture, I gratefully acknowledge that I have received in return all that a man could justly aim at. My satisfaction in this respect is not a little enhanced when I look back to the number of zealous and able men in whose education I have been enabled to assist, and who are now occupying, in various countries, a distinguished position in the front rank of Science, and are, with splendid success, cultivating and extending her domains—teaching, diffusing, and successfully applying those principles of investigation which constitute the true foundations of scientific progress. It is with pride that I am able to add that in these my former pupils I have gained an equal number of warm friends, who, I am sure, look back with pleasure to the time when we combined our powers in one common aim and effort.

"And now, in addition to all that a benevolent destiny had already granted me in measure above many, I receive from my friends in England, in this gift of friendship, in this testimonial of honour, a token and a proof of their recognition and approbation of my labours.

"When I reflect that whatever of good a man accomplishes flows from an inner impulse of which he is often but imperfectly conscious, and that a higher power has a part in all his labours and usefulness—giving to them their life-germ and their capacity of growth, I must own that in receiving this noble Present I am blessed far beyond my deserts.

"I feel myself in the highest degree honoured and most deeply touched by this substantial and permanent expression of the kind feelings of my friends in England. Convey to them all my best and warmest thanks. This Gift of Honour possesses for me inestimable value, and will remain a lasting memorial in my family. "Dr. Justus von Liebig.

"To Thomas Graham, Esq."

In his private life Liebig's character showed the

same simplicity and nobility that distinguished him in his relations with Faraday, Wöhler, and Dumas. His intercourse with his pupils was marked, they tell us, by a dignified composure combined with a marked simplicity and kindness which could not fail to encourage the most timid beginner; whilst he gave to the assiduous worker a degree of helpful sympathy which led him to make any sacrifice, and lasted long after the period of personal intercourse. Nor did he reserve his interest and support for his own pupils alone. His generous instincts, which led him to help these so unstintedly, impelled him also to give counsel or assistance wherever he recognised that it was deserved. Thus, when Sir Henry Roscoe was a candidate for the professorship of chemistry at Owens College Liebig—who had become acquainted with Mr. Roscoe on the occasion of one of his visits to England—hearing of his candidature at a moment when he was about to depart on a journey, when the carriage was, indeed, standing at his door, at once delayed his departure in order to write a few words in support of the young Englishman, of whom he had formed a high opinion.

One more illustration of the generous disposition which prompted Liebig to help, whenever he saw that help was needed, whether by a friend or by a stranger, will be welcomed, as it shows so well his genuine goodness of heart.—Shortly after his call to Munich (1853), Liebig, in company with Hofmann and two other friends, was making, as Hofmann has told us, an excursion among the mountains of the Tyrol.

One morning, in the course of their walk they came upon an old soldier, wasted by fatigue and enfeebled by disease, a pitiable object, travelling slowly

along. As they came up to him he accosted them with a piteous tale, asking their aid. Liebig, whose purse was always as readily opened as his heart, and his friends soon made up a little stock of money for this poor wayfarer, which doubtless seemed to him a small fortune. They, passing on, soon reached the next village where they were to rest and dine, leaving the traveller to follow at his slower pace, and presently he, too, was observed to enter the inn. Their own meal finished, his benefactors settled down to a *siesta* before continuing their journey, soothed by the reflection that for once the poor soldier also had the means to procure himself a substantial meal.

Half an hour later Hofmann awoke. But where was Liebig? There were his two fellows fast asleep. But where was their leader? Surprised by his disappearance, Hofmann at once got up and inquired of the landlord what had become of his friend, "the elderly, spare man of the party." The landlord told him that a little while before the gentleman had inquired for a pharmacy; and on being told that there was none nearer than that in the next village over the hill, he had set out in that direction. By no means without anxiety Hofmann forthwith set out along the road Liebig had taken, and presently saw his figure on the brow of the hill. Hurrying forward, he soon met him, and then learned that he had noticed in the soldier symptoms of low fever, such as quinine was certain to cure, and had been to the nearest pharmacy to get some.

On his arrival he had found the pharmacy closed, and the apothecary away, but his wife had given Liebig the run of the place and allowed him to take what he liked and fix his own price. Fortunately

quinine was in the stock, and so his object was secured, and soon the wayfarer was provided with a box of powders which were probably sufficient for his cure.

The incident was simple enough. But how many of us at fifty would have undertaken this extra tramp, after a midday dinner and a long walk, with another walk before us, in order to afford succour to a wayside beggar? Could even those whom Liebig belaboured in the arena of scientific controversy help loving such a man when they came to know him, or fail to forgive him if in his ardour in the support of what he considered to be true, he sometimes exceeded the bounds of courtesy in scientific warfare?

Liebig was probably never what we should call a robust man, though he possessed undoubtedly very remarkable powers of sustained work of the most exhausting kind. In his letters to Wöhler complaints of ill-health were certainly frequent; but from the amount of work he got through, it seems likely that he suffered rather from occasional *malaise* than from actual illness. For many years it seems pretty certain that he was always somewhat overworked, for though he was frequent in his injunctions to his friend Wöhler to "throw writing to the devil," he himself never failed to respond to any call that was made upon him to do work of the literary kind. And we know, too, from Wöhler's letters that the discussions in which he shared were no play to him, but, while they lasted, grim reality, and a source of considerable strain, which told upon his physical health. We must not regret this, however. Our science would have been less forward to-day if Liebig had not thrown his whole soul into those battles of the giants which distinguished the middle of the present century.

After twenty-eight years at Giessen, Liebig was called to Munich in 1852. And there, in spite of an invitation to Berlin in 1865, he worked till the end. At Munich Liebig made no fresh great departures, but he continued to work, following the lines he had already laid down. The change from quiet Giessen to Munich seems to have been in every way grateful to him. When he arrived there, he found the new laboratory was barely roofed in, he was without materials and apparatus, and there were new assistants who had to be trained to his ways; but, in spite of all difficulties, he was soon able to start his first course of lectures, which was attended by two hundred and fifty students, and at Christmas he declared, in a letter to Wöhler, that "he had made a good exchange."

Liebig was *persona grata* at the Court of King Ludwig, and at an early day was called upon to lecture before his Majesty and some members of the Royal Family. There was an explosion; it looked like a bad one, and Liebig, as he recovered from the effects, was horrified to see blood streaming from the faces of Queen Theresa and Prince Leopold. Fortunately their injuries were trivial; and their agitation soon subsided when they found that Liebig, who, of course, had been in the thick of it, was also not seriously damaged.

The advantages gained by the change to Munich were twofold. First, there was the wider social life, in which Liebig found much pleasure. Secondly, he was no longer called upon to personally superintend the work of large classes of students in practical chemistry. And, besides, the greater leisure which he found in his new post made it possible for him to enjoy, more freely than before, the pleasures to be found in a cultivated society, and to indulge a marked taste for

general literature, which we first hear of in connection with the correspondence between Liebig and his friend Platen.

As the years rolled by, Liebig, in spite of his profound interest in his work in applied science, now and then looked back with regret on the early days at Giessen, when he laboured chiefly in pure science. When discouraged, as he sometimes was, by the misconceptions of his opponents, or by the slowness with which many physiologists and agriculturists learned what he had to teach them, he was now and then tempted to envy Wöhler, who had not plunged with him into these troubled waters, but remained to the end constant in his devotion to the study of chemistry. It was in such a mood that in 1857 he wrote to Wöhler—

"Your letters of the 5th and 14th affect me like a story of the old times; there is the old fire and youth, and years which are gone and tones which have sounded rise up and transport me to the early days of our happy collaboration. You have kept your single-mindedness, and have enjoyed ever-renewed happiness; but I seem to myself like a renegade, a deserter, who has given up his religion and has nothing in exchange. I have left the paths of science, and, in my efforts to be of some use to agriculture and physiology, I am rolling the stone of Sisyphus, which ever falls back on my own head, and I despair sometimes of the possibility of getting it on a firm foundation. . . ."

Again, in the same year, he wrote :—"I admire you and your beautiful researches. How happy you are in your province! You are older than I, and yet I am much less bright than you. You seem to me in

your work like the man in the Indian story, from whose mouth bunches of roses fell whenever he laughed; I, with the agriculturists, am condemned by fate to bear water to the cask of the Danaids. All I do is in vain. I trouble myself and consume my best powers without gaining any result; I have not won a single voice for me and my principles by the Chemical Letters."

It is needless to say that these expressions were but the outcome of passing moods, and that, upheld by a real inward conviction, Liebig never ceased to declare what he believed to be the true lessons that chemistry offered to agriculture and physiology. Nor was he, as time went on, without abundant evidence that his labours were appreciated; for, besides the ordinary honours, which were literally showered upon him, within a single year (1865) a call to Berlin would have been his at a word, he was invited to visit England at the cost of Parliament to give evidence on the utilising of town sewage, and received a vote of thanks from the Corporation of London for the assistance he had given in regard to this difficult subject: though it is to be feared that, in this case, he must have been disappointed with the immediate result of his efforts, for we know he hoped that England, with its vast wealth, was about to set an example to the other countries of Europe, and to teach them how to make use of the vast stores of invaluable plant-food which, under the name of sewage, is daily cast irrevocably away by the great cities.

In 1867, after much hesitation, Liebig consented to go to Paris as one of the Presidents of the great Exhibition. In spite of his hesitation, this visit proved a rich and great joy to him, as he told Wöhler

afterwards. The renewed intercourse with such veterans as his old friends Deville, Frémy, Wurtz, Péligot, and Chevreul was delightful. During his visit he dined with Napoleon, whom he commended afterwards as one who "could not only talk, but listen." In exercising his function as a president, he was much struck by the want of order in the French committees, where the members did not address the president, as in Germany and England, but each other. This made his work difficult sometimes, but his "*vice*" helped him out, and so he got on pretty well. It is interesting to notice that in this year Liebig, for the first time, dismissed an assistant. When he wrote at the time to ask Wöhler to find him a new one he paid a handsome compliment to the pharmacists, saying that he would prefer "a pharmacist, who is accustomed to order, cleanliness, and has a sense of duty."

A large proportion of the numerous letters which passed between Liebig and Wöhler, and of those between Liebig and Berzelius, have been published since Liebig's death—the former under the editorship of the late Professor Hofmann, who was assisted by Fräulein Emilie Wöhler; the latter under that of Justus Carrière. The earlier letters dealt very largely with their scientific work, and will afford a happy hunting-ground for future historians of chemistry, whilst his correspondence with Reuning (1854–73) will be not less interesting to agriculturists. His later letters to Wöhler, especially those written from Munich, and some of the others were, however, much less exclusively scientific. These throw a clear beam of light, by which one gets some pleasant glimpses of the simple and happy home-lives of these two men,

and of their deep affection for each other. Very many of the quotations already given are taken from the letters of the Munich period. Individually these letters are, most of them, very simple. Many relate to little gifts of *bock* or cigars from one to the other, or consist of discussions of plans for holidays, or of short descriptions of past excursions, and such like matters. In one we find Wöhler's daughter Fanny at Munich taking care of Liebig, and rendering "invaluable" services by helping him to manufacture bread on chemical principles. Several give us little domestic details of this kind. Thus one shows us something of the family anxiety for the fate of Dr. Georg Liebig, who was in India during the Mutiny, but who escaped, and not long afterwards came home, married, and settled in the Fatherland. From another we learn that all, or nearly all, Liebig's grandchildren were the subjects of successful experiments with his "infant's food," which is still often found valuable by the doctors. In a few—a very few, happily—we hear of visits from the Angel of Death. The frequent references to his health make it clear that in the later years Liebig's work was often done in the face of great difficulties. More than once his yearning for the sight of his old friend's presence shows itself, and we find him giving voice to the wish that his few remaining years could be passed in Wöhler's company. Early in 1861, when he was sleeping badly, he wrote: "If only I had the old wish to work, the old power to overcome difficulties! Write to me more often. Your chatter gives me a cheerful day. When Pfeuffer comes to me, he says to me frequently, 'Ah, you have had a letter from Wöhler!'"

In 1870 Wöhler, for the last time, proposed a new joint research. But Liebig refused; he declared himself unequal to what Wöhler proposed. "With my last," he says, "I concluded my course." During 1871 his health was very poor, and it is plain that he then sometimes felt the end was not very far off. On the last day of this year he wrote to Wöhler the following touching words—

"I cannot allow the year to pass without a sign of my continued existence. We shall not for long be able to send each other these happy wishes for the new year; but even when we are dead, and have long been dust, may the bonds which have united us ever keep us both in the remembrance of men as a not too frequent example of two men who were faithful, without envy and ill-feelings; who wrestled and strove in the same arena, and were ever firmly knit in friendship."

But though Liebig felt that he must now abandon the hope of doing much more himself, his generous, yet patriotic, address to the Bavarian Academy after the close of the Franco-Prussian War shows us that even in 1871 he still retained in a high degree his old vigour and fire, and that, if he was sometimes saddened by the thought that his own work was so nearly done, he was still full of hope for Germany and mankind.

During the winter of 1872 he continued to lecture as usual; in January, 1873, he even wrote that his lectures were a refreshment to him; and in the following month he was well enough to be trying experiments on the making of cyanogen and on the feeding of pigs. But the end was now close at hand.

Liebig's last letter to Wöhler was written on April 3rd of this year, and the last letter he received from Wöhler was written on the 7th day of the same month. Liebig wrote:

"MUNICH, 3rd *April*, 1873.

"I intended to have written to you yesterday, but I had a bad night, without any sleep, and the whole day I lay tired and exhausted upon the sofa. I thought of you—your good sleep, your good appetite, the normal activity of all your functions. Would it be possible to perish in old age merely through sleeplessness, without sickness? It is the vegetative life, the recuperation in the night. When this fails, the lamp is gradually extinguished. I was not well a single day in Wiesbaden, and feared to sojourn in the low ground; also many other things did not agree with me there. I should not be averse to go to Hanau to your brother-in-law. We might afterwards pass a few more days in the Bavarian Mountains. My plan is to take leave from Easter onward, and to do nothing for the half year. I have a great wish to go to Vienna; from thence to Magdeburg, to Rimpau; then to Hamburg and Kiel, to Meyer's. This is to hope that you will come with me. What do you say? I shall not go to a cure. It was no use to me last year; and in Wildbad, for instance, walking through the streets to the bath is not agreeable.

"We have heard with the greatest grief of the death of your sister-in-law in Berlin: she has been, I hear, suffering for some time. It is certainly my friend, General Hartmann, who is dead, the same who was with us in Reichenhall. He went to a funeral in winter, and thence, as in so many cases, contracted his fatal illness.

"With heartiest greetings to Fanny,

"Your faithful

"J. VON LIEBIG."

Wöhler was not well enough to attempt to travel, however, and on the 7th he replied as follows. This was the last letter he wrote to Liebig:

"WIESBADEN, *April* 7th, 1873.

"My best thanks for your letter of the 3rd. It is now a year since we have seen each other, and it would distress me very much if it should not come to pass that we meet this Easter. But I fear extremely that I must resign myself to this. I could not come to you this time, since I have first to consider the full restoration of my strength, which I hope to attain in this mild climate by a completely quiet life. I still hope that you will accept the pressing invitation of my brother-in-law, and that we may meet in Hanau; whereto your remark, 'I am not averse to come there,' encourages me. You certainly seem inclined for travel, but I am much surprised that you wish to go to Vienna, and to rush into this disagreeable, unsatisfying, exciting spectacle, the Exhibition.

"A few days ago Buff with his wife took us by surprise; they stayed here two nights, and then journeyed to Crefeld, to their son Henry.

"In answer to my questions, Lipsius writes that the eagles on the captured French flags really consist of gilded aluminium, a metal that was first produced in Berlin in 1827. *Sic erunt fata.*"

The visit to Hanau never took place. Liebig and Wöhler met no more, for Liebig died at Munich on April 18th, 1873.

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